

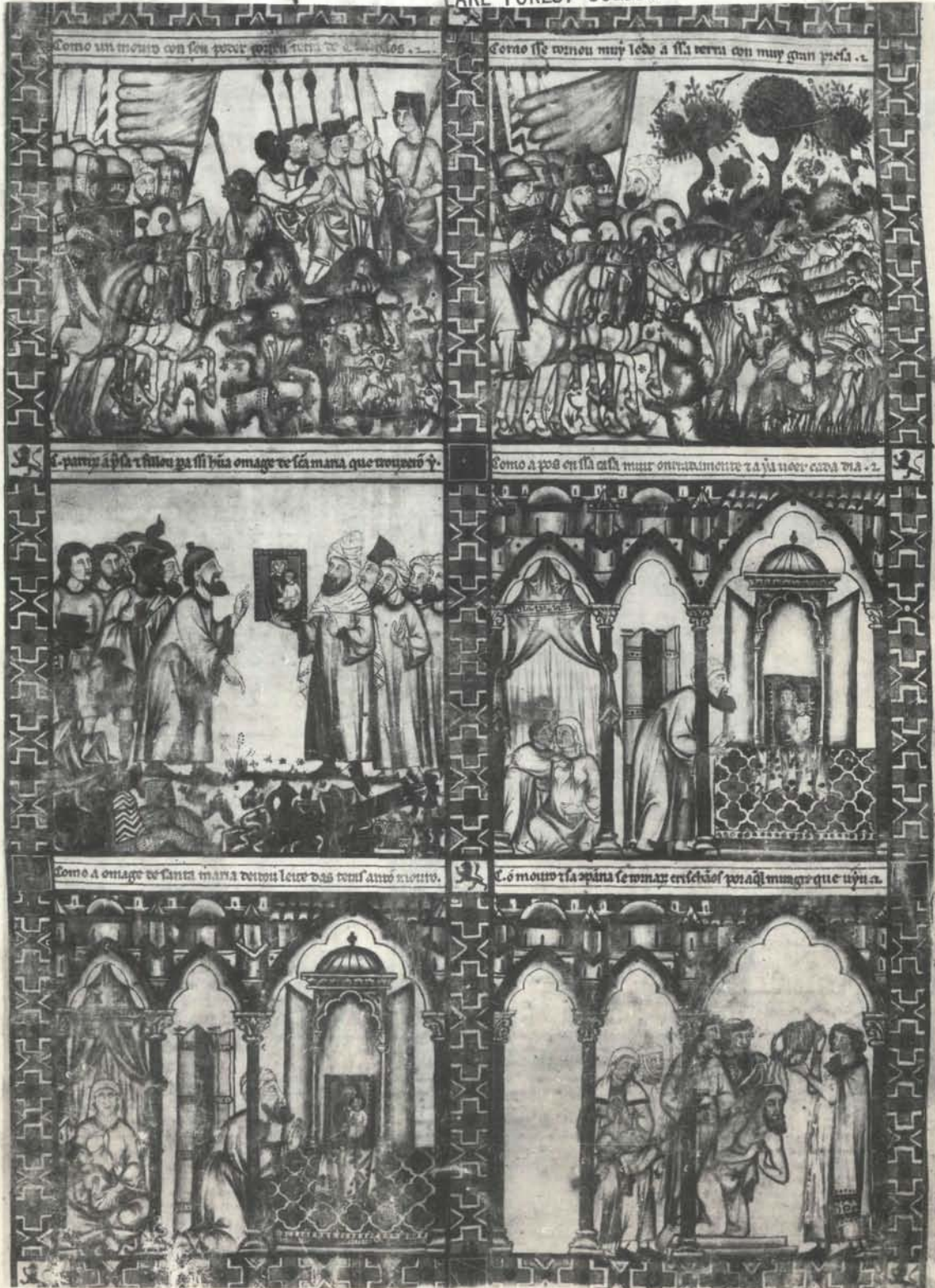
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On War and Greed in the Second Century B.C.

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THE PROBLEM of the possible economic motives of Roman imperialism has rightly seemed to nonspecialists one of the most important in the history of the Roman Republic, but it has for a long time received relatively little attention from specialists.¹ One kind of economic explanation, in terms of markets and opportunities for investment, was attacked long ago by Tenney Frank in what remains the classic discussion;² his arguments were later improved by others, and, as far as most Roman acts of expansion are concerned, explanations of this type have today little support among Roman historians. For a long time there were no extended discussions of the possible economic motives for Roman imperialism in the second century B.C.,³ but recently a leading student of the external relations of republican Rome, Professor E. Badian, returned to the problem, and he came to decisive conclusions. In his view, "no such motives can be seen, on the whole, in Roman policy"; "strange as it may seem to a generation nourished on Marx, Rome sought no major economic benefits"; "the whole myth of economic motives in Rome's foreign policy at this time [the second century] is a figment of modern anachronism, based on ancient anachronism."⁴ It is emphatically denied that desire for economic gain, public or private, was a motive that influenced Roman decisions to go to war, and those who have denied this are dismissed, not without justification, as having imposed doctrinaire views of imperialism on a set of facts that they do not fit. It can be said more generally that competent ancient historians are nowadays acutely aware of the dangers of anachronism in explaining imperialistic behavior in the ancient world; one notes the strictures of Professor K. J. Dover on those who have tried to find

¹ I use the term "imperialism" here to mean expansion of power in general, not only annexationist expansion; cf. below, p. 1382. In this article I have kept footnotes and discussion of the numerous peripheral controversies to a minimum.

² *Roman Imperialism* (New York, 1914), especially 277-97. This is mostly repeated from his "Mercantilism and Rome's Foreign Policy," *AHR*, 18 (1912-13): 233-52.

³ Filippo Cassola, *I gruppi politici romani nel III secolo a.C.* (Trieste, 1962), especially 50-83, 393-404, made some interesting remarks about the third century, and also about the second. For a critique see especially the review by John Briscoe, *Classical Review*, n.s. 13 (1963): 322-23.

⁴ *Roman Imperialism in the Late Republic* (2d ed.; Oxford, 1968), 17, 18, 20.

the "real motives" for the Athenian expedition to Sicily in 415 outside the pages of Thucydides.⁵

In the Roman case some contemporaries of the events certainly seemed to think that there were some motives that can be called "economic." When in 88 B.C. King Mithridates of Pontus captured a Roman general and ex-consul, Manius Aquillius, he gave a startling demonstration of what some of their more vigorous opponents thought of the Roman imperialists: Aquillius, who had perhaps offered special provocation, was put to death in a symbolically appropriate manner, by having molten gold poured down his throat. Thus was greed rewarded. It is probable in fact that Roman greed by this time played an important part in the propaganda of the Hellenistic enemies of Rome. It may of course be the case that such accusations were completely unfair⁶ or resulted from effects of Roman expansion that were purely incidental.

Again, no one doubts that by the sixties of the first century B.C. the desire of senatorial politicians to pay off their debts and to enrich themselves was an important motive and possibly the main motive of the bellicose attitude of Rome toward some foreign states. We have much less firsthand information about Roman politics and finance in the second century. Conditions were different then—but how different?

The purpose of this article is to argue that while modern scholars have succeeded in showing that certain kinds of economic motives, which can be roughly classified as mercantilist or commercial, were generally of very slight importance for Roman imperialism in the second century, they have failed to pay sufficient attention to other economic advantages that accrued to those Romans who decided what foreign policy would be. Desire for these other economic advantages was, it will be argued, an important motive in the formation of Roman policy.

WE SHOULD ADMIT STRAIGHTAWAY that since we can only speculate about the unconscious reasons why Roman senators behaved as they did, there can never be any completely satisfying explanation of their behavior. If we restrict ourselves to more or less conscious motives, we shall still have a difficult time with the evidence. There were reasons to distort in public what may have been the real motives for action. When the Romans went to war they almost invariably felt that it was necessary to satisfy the formal requirements of the fetial law, according to which war could properly be fought only for *res repetitae*, to obtain compensation for wrongs suffered. It is clear too that the high regard felt for certain virtues, particularly *fides* ("good faith") may also have seriously distorted stated reasons

⁵ A. W. Gomme, A. Andrews, and K. J. Dover, *A Historical Commentary on Thucydides*, 4 (Oxford, 1970): 229–30.

⁶ Not that such views were restricted to those foreigners who were hostile to Rome; cf. 1 Maccabees 8: 3.

for going to war. Polybius in fact tells us explicitly that when they went to war the Romans were careful to find a pretext that would appeal to foreign, that is to say mainly Greek, opinion.⁷ Further, the restraints sometimes suggested by *fides*, and perhaps even those of the fetial law, may have had a real effect on policy. It is rash to assume that religious and moral beliefs of this kind are only camouflage. Evidently in the time of Polybius some, if not all, members of the Roman Senate were quite cynical about the fetial law, but it would be wrong to assume that it had no effect in determining the occasions on which Rome went to war. Finally, the character of the Roman political system made it difficult for ancient writers, and now makes it difficult for us, to know what arguments were used in favor of, or against, any particular decision to make war. Cassius Dio tells us that it was difficult to write the history of Rome under the Empire partly because the most important decisions were made in private,⁸ and (whatever Dio thought) it was not much easier to write the history of the Republic. Serious discussions about foreign policy did take place in the Senate (not that any official record of the proceedings was made in the second century), and the will of the plebs, or at least part of it, could sometimes make itself felt even in matters of foreign policy (see below)—but in general, as is universally agreed, decisions of foreign policy were effectively in the hands of a rather small number of influential senior senators who had no need to broadcast full and candid accounts of their reasoning.

None of our main sources attempts to assess the possible economic motives of Roman imperialism. Polybius' generally favorable attitude toward Rome appears at its strongest (at its most defensive, some might say) on the subject of financial probity—with at least one demonstrable exaggeration as a result. He knew that the Romans had sometimes done more plundering than was required by their assumed aim of obtaining political domination over the whole world; we are prevented by textual gaps from knowing whether he had any explanation of the fact. Later on in the writing of his history, however, even Polybius came to think that the financial probity of the Romans had declined since they had begun to fight "overseas wars."⁹ Later writers about the second century were almost all believers in theories of a moral decline, marked by *luxuria* ("self-indulgence") and *avaritia* ("greed"), which was supposed to have invaded the Roman state from various dates, mostly in the period 187–146. The intellectual and political history of this outlook is a matter of great interest, but it cannot be pursued here; it is enough to point out that *avaritia*, which was in the view of some later Roman writers the great charge that could be leveled against Roman imperialism, was schematically excluded from explanations of the earlier

⁷ Polybius 36. 2.

⁸ Cassius Dio 53. 19. 3.

⁹ On financial probity see Polybius 6. 56. 1–5—but we know of no one who was put to death for electoral bribery. On the excessive plundering of Syracuse in 211, see Polybius 9. 10. Polybius' later views are in 18. 35. 1–2; cf. 31. 25.

phases of Roman imperialism. Such motives were thought by, for example, Cicero and Sallust to have been foreign to the Roman aristocracy before certain dates (it is possible of course that they were right); the scheme was pervasive, and it was not thought necessary to produce evidence.

What economic effects of war were visible to second-century senators? I will deal first with public finance. It is very difficult to reach any satisfactory estimates of the actual effects of war—on the one side indemnities, booty, real property (including mines) acquired by the state, and provincial taxation; on the other side primarily the expenses of maintaining military forces. Some scholars, notably Tenney Frank, have attempted to calculate the public expenditure and income of Rome in this period.¹⁰ The results are inevitably inconclusive—even for the period down to 167, for which Livy provides much relevant information—mainly because certain items of income, for instance provincial taxation, booty from some sources, and the income from the Spanish mines, cannot be worked out within useful limits. We do not, incidentally, have any evidence to justify the common assertion that Sicily was the only province that “paid its way” before 133. Nor can any conclusion about the proportion of public income to public expenditure be drawn from the information, provided by the elder Pliny, that in 157 the treasury contained gold, silver, and coin to the substantial but not spectacular value of 104 million sesterces, for there would have been little point in building up a reserve significantly larger than that.¹¹ Unfortunately the figures that Pliny gave in the same passage for the state of the treasury in 91 are not properly transmitted in the manuscripts; it certainly looks as if they showed an immense accumulation since 157, but no safe conclusion can be drawn.

There are, however, two points that can usefully be made about public finance in the second century. First, its whole scale was much greater than it had been before the Second Punic War, so that even if income exceeded essential expenditure by only a fairly small percentage (as Frank concluded—on insufficient evidence—for the period 200–157), much larger sums were now available for nonessential purposes. Second—and this is particularly important since it was evidently noticed by senators, as the precise effects of empire on the treasury may not have been—the Romans were able in the second century to afford some public luxuries unimaginable in the third century. No one will suggest that the Romans fought wars primarily to free themselves from their light burden of direct taxation, but the fact remains that in 167, when Aemilius Paullus brought home from Macedon booty to the value of some 120 million sesterces (or quite possibly a much larger sum) and Macedonian taxation began to flow into the Roman treasury, direct taxation of Roman citizens ceased. Equally, no one will suggest that

¹⁰ *An Economic Survey of Ancient Rome* (hereafter *ESAR*), 1, *Rome and Italy of the Republic* (Baltimore, 1933): 126–46, 222–31.

¹¹ Pliny *Naturalis Historia* 33. 55.

the Romans fought wars in the second century primarily to provide themselves with public works that they could not have afforded before, but the fact remains that by previous standards it was a time of immense expenditure on such things. After a period of understandable restraint in the first three censorships after the Second Punic War, there began with the censorship of the elder Cato and L. Valerius Flaccus in 184 a period of vigorous activity. Frank collected most of the details,¹² and they need not be repeated here, but since so little attention is usually given to the financial implications of these activities, I will mention some important instances. As a preliminary it may be helpful to point out with all due reserve some indications of the scale of Roman public finance in this period. According to the best estimate, the ordinary annual income from direct taxation of property of Roman citizens (the *tributum simplex*) did not in the period of the Second Punic War exceed 3.6 million sesterces, and may have been even less.¹³ The cost of the pay of a single legion for one year was nominally about 2.4 million sesterces (but many legions were not kept in service for a full twelve months, and deductions from cash pay were made for food and equipment). In their censorship Cato and Valerius Flaccus probably spent 6 million sesterces (exactly the amount that King Antiochus was paying in indemnity each year) on a single project, the improvement of the sewage system. Livy provides a long list of other projects, as he does for the censorships of 179 and 174. The censors of 174 undertook building not only in the city of Rome, the normal practice, but in colonies as well; it has been suggested that this was done at the expense of the colonies themselves,¹⁴ but it is fairly clear from Livy's account that the expense was borne by the central treasury.¹⁵ In 144–140 the Aqua Marcia, the greatest of the Roman aqueducts up to that time, was constructed at a cost of 180 million sesterces. It was also a great period of road building: not to mention roads in Macedon and Spain that had not been needed before, most of the great trunk roads of Italy were constructed in the second century, as is now becoming increasingly clear.¹⁶ Building styles were also becoming more luxurious. In 146 the first marble buildings in Rome—the temples of Juno and Jupiter vowed by Q. Caecilius Metellus, the conqueror of Macedon, and paid for from his spoils—were erected, and the first gilded ceiling was constructed in the Capitolium in 142–141.

Scholars have often been puzzled by the emphasis that Polybius in his description of the Roman political system places on the importance to the *demos* of the state contracts for building and revenue collecting—almost

¹² Frank, *ESAR*, 1: 183–87.

¹³ See Gaetano De Sanctis, *Storia dei romani*, vol. 3, pt. 2 (Turin, 1916): 623–31.

¹⁴ Frank, *ESAR*, 1: 186.

¹⁵ Livy 41. 27. 10–13. (On the text, compare Will Richter, "Zum Bauprogramm der Censoren des Jahres 174 v. Chr. [Livy 41, 27, 5–12]," *Rheinisches Museum für Philologie*, n.s. 104 [1961]: 257–69).

¹⁶ Compare my *Rome in Etruria and Umbria* (Oxford, 1971), 161–69.

everyone, he says, was involved in them.¹⁷ That was not of course literally true, but the statement is only intelligible if we pay attention to the enormous growth of public building activity in the second century, growth which was in effect financed by war and conquest.

There were still further public luxuries: while Gaius Gracchus' grain-subsidy law of 123 would have been inconceivable for other reasons in the third century, it could not in any case have been afforded then. There had been selling of grain below the market price at least once in the third century (in 251), but for a regular scheme the treasury had to be as affluent as it was in the second century. We do not know at all reliably what the average market price of grain was at Rome in the time of Gaius Gracchus, or how many people benefited from his scheme (some three hundred and twenty thousand people eventually seem to have benefited from the later scheme of Clodius); but it can be estimated that for every ten thousand beneficiaries the state expended in an average year from seven to eight hundred thousand sesterces. Right-wing persons later claimed that Gaius Gracchus' law exhausted the treasury, which was quite untrue; but the expense was considerable and again, in effect, financed by war and conquest.

These facts are known to the scholars who deny that economic motives were of any importance in leading Rome into war. They generally argue that Rome did not put much effort into exploiting her overseas power for public revenue. Much attention is paid to the fact that under the terms of the Roman settlement in Macedon in 167 Rome collected taxes at somewhat less than half the rate of the taxation of the Macedonian kings. This is not, however, to be explained by a lack of will to exploit on the part of the Romans. It may very well have been the case that the Senate estimated that no higher rate could be exacted without the presence of a Roman governor and a regular garrison (Macedon had not been annexed), which was undesirable for other reasons.¹⁸ There is no reason to think that the total burden of taxation paid by the Macedonians was lightened, for although they no longer had to pay for King Perseus and his army, they did have to pay (after they had suffered the devastation and pillaging of war) for the governments of four republics and, in three of them, for some military forces. Nor should we suppose that because the Senate decided to close the Macedonian mines in 167 and forego the revenue from them it was indifferent to this revenue; relations between the Senate and the *publicani* who might have organized the collection of revenue were bad at this time, and it would obviously have been dangerous to leave collection in local hands; in any case the mines were reopened in 158. It begs the question, I think, to say that increased revenues were merely an incidental result of war. Rome is said

¹⁷ Polybius 6. 17. 3-4.

¹⁸ I shall be dealing elsewhere with the Senate's supposed reluctance to annex territory in the second century, which is heavily emphasized by Badian, *Roman Imperialism*.

to have continued to collect revenues in the provinces “as much from inertia as from conscious choice,”¹⁹ a phrase that I confess I do not understand. The decision to collect revenues was indeed utterly unsurprising, but there is no reason to suppose that it was anything other than entirely conscious.

Roman wars were wars of plunder, at least in the sense that plundering was a normal part of them.²⁰ If prisoners were lucky enough to survive, as a general rule they had to be ransomed or sold into slavery. Moveable private property—very widely defined—came into the possession of the victorious army, and, whatever the precise legal status of such booty was, a share was seldom in practice refused to the soldiers. About the effects of this on ordinary soldiers I shall have more to say later. Officers, including those of senatorial rank, received a proportionately larger share. What army commanders took for themselves is, rather oddly, less clear. There were some famous instances of self-restraint, but they help to prove that self-restraint was not the normal practice of victorious commanders.²¹ When the elder Cato commanded in Spain in 195–194 his army acquired a large amount of booty, but, says Plutarch, Cato himself took none of it, except what he ate and drank. Plutarch attributes a characteristic saying to him: “I do not blame those who seek to profit from such things, but I wish rather to strive in bravery with the bravest than in wealth with the wealthiest or in greed with the greediest.”²² (For Cato’s business interests, see below.) Polybius mentions two cases of such self-restraint: after the battle of Pydna in 168 the whole kingdom of Macedon and its treasury came into the power of the Roman commander Aemilius Paullus, but, says Polybius, he did not desire any of this booty or even wish to look upon it. After the capture of Carthage in 146 his son Scipio Aemilianus took nothing for himself.²³ But there were different levels of self-restraint. It was quite a common practice for victorious generals to spend at least part of their own share of booty on temples or other public purposes, with obvious benefit to themselves. Cicero praises L. Mummius, the commander of the Roman army that performed one of the most predatory and barbaric acts of late republican history, the sack of Corinth in 146, for his self-restraint with regard to booty. In this case it can be seen how self-restraint could be shrewdly combined with self-promotion: Mummius did not take booty for himself, but he “adorned” Italy and the provinces with it, as inscriptions commemorating his gifts to various towns also attest.²⁴ It is evident in fact that army commanders normally took a

¹⁹ Badian, *Roman Imperialism*, 18.

²⁰ The essential discussion of Roman law and practice concerning plunder is that of K. H. Vogel in Pauly-Wissowa, *Real-Encyclopädie*, s.v. *praeda* (1953), cols. 1200–13. André Aymard, “Le partage des profits de la guerre dans les traités d’alliance antiques,” *Revue historique*, 217 (1957): 233–49, reprinted in his *Etudes d’histoire ancienne* (Paris, 1967), 499–512, is also important.

²¹ Note particularly Polybius 31. 22. 3.

²² Plutarch *Cato Maior* 10.

²³ Polybius 18. 35. 4–5 and 31. 22 (Aemilius Paullus); 18. 35. 9–12 (Aemilianus).

²⁴ See Attilio Degraffi, ed., *Inscriptiones Latinae Liberae Rei Publicae*, 1 (2d ed.; Florence, 1965): nos. 327–31.

substantial share of booty for themselves, though a commander with other ends in view, renown or popularity, might abstain in a particular case or use the whole of his personal share for pseudopublic purposes.

After provinces were annexed governors had large opportunities for profiteering, and the opportunities were sometimes, perhaps regularly, taken. Attempts were made by the Senate, or at least with the Senate's approval, to limit this kind of exploitation, but the restraints were never at all strong, and there is no means of knowing what the Senate regarded as the reasonable limit of exploitation. In 171 envoys from some allies in the Spanish provinces complained to the Senate about the greed and cruelty of some Roman magistrates. Judges were appointed to hear charges against three officials, and the affair ended in one acquittal and the voluntary and not very arduous exile of two of the defendants in Latium. Evidently no money was recovered by the Spaniards. The contents list of the lost forty-seventh book of Livy indicates that it contained an account of how in the period 159–154 several praetors were accused by provincials of *avaritia* and condemned. L. Cornelius Lentulus Lupus, consul in 156, was convicted of a *repetundae* (extortion) charge, and in 149 an unsuccessful attempt was made in a famous case to convict Ser. Sulpicius Galba, a former governor of Spain, of the same charge. There followed in the same year the *Lex Calpurnia*, which set up a permanent court, with a senatorial jury, to deal with such charges, an action that has recently been interpreted, convincingly in my opinion, as an attempt to take such proceedings into the gentle hands of fellow-senators rather than as evidence of increased concern for the interests of provincials.²⁵ The *Lex Calpurnia* probably did have a restraining effect on provincial governors, but known convictions that genuinely arose from the economic exploitation of the provincials were rare. Such convictions could be obtained only in very exceptional circumstances, and in general the sole controls over a governor's behavior in this respect were the informal disapproval of his peers and his own conscience. Even though the egregious Verres showed such remarkable energy in exploiting Sicily when he governed it that the consuls of 72 tried to restrain him, he afterward had his governorship renewed by the Senate. No one familiar with the more detailed evidence about the behavior of governors in the first century B.C. can be confident that in the second century more than a handful of governors ever conformed to Cicero's idealistic maxim, according to which a good man should bring back from abroad one thing only—good repute.²⁶

It is impossible to judge the extent of the business interests of second-century senators. Most scholars seem to think that senators had few such interests beyond those that arose directly from their ownership of land, and I do not want to press the small amount of evidence to the contrary. The

²⁵ E. S. Gruen, *Roman Politics and the Criminal Courts, 149–78 B.C.* (Cambridge, Mass., 1968), 13–15.

²⁶ Cicero *De Legibus* 3, 8, 18.

Lex Claudia of 218, which limited the size of ships that a senator or a senator's son could use in trade, was apparently forced on a reluctant Senate. It turned out to be possible to evade it. The maritime loans of the elder Cato are often thought to have been exceptional and a mark of a "new man" in the Senate,²⁷ but it is hard to be confident that the rigid moralist Cato was less strict in abstaining from commercial activities than the generality of other senators. Senators were also excluded from direct participation in state contracts,²⁸ and thus from one means of profiting from war and empire; whether this rule too was evaded we do not know. Most of their wealth probably was in fact in the form of land. (It is likely that senators were already beginning to acquire estates in some of the provinces in the second century, but it cannot be proved.)

Did Roman governments pursue particular foreign policies designed to benefit large-scale Roman landowners? The ban that was placed on the growing of olives and vines in Transalpine Gaul, probably in 154,²⁹ has sometimes been cited as an instance of such a policy. It is now usually interpreted instead as a favor to Rome's allies at Marseilles, but even if that interpretation is correct, some landowners in Italy may also have benefited in an appreciable way. In one respect, however, all senators, like other men of property, benefited from victorious wars and cannot have helped noticing the fact. Such wars greatly increased the supply of slaves, the best sort of labor for farming and many other purposes. It is of course the error of Marxist theoreticians, as it was also J. A. Hobson's,³⁰ to single out the slave supply as the one factor that really mattered, a belief for which there is no evidence whatsoever. There certainly was a tremendous influx of slaves, so great that it indirectly threatened to disrupt the whole Roman social and political system. There are no statistics for the slave population, but the market (dominated by Roman buyers, though not confined to them), which could absorb one hundred and fifty thousand slaves from Epirus at a single time in 167, was enormous.³¹ It is important to state also that the enslavement of the Epirotes was a gratuitous act—there does not seem to have been any political reason for it. Even Tenney Frank had to admit that this act "might support an inference that the Senate was eager to provide cheap labor in Italy."³² Appian describes the situation in Italy: the rich built up their *latifundia* and used slaves on them as farm laborers and herdsmen, since free labor would have been drawn off from farming into the army. At the same time the ownership of slaves brought them great profit because of the fertility of the slaves [this is questionable]. Thus the powerful became extremely rich and the race of slaves multiplied throughout the country,

²⁷ Our information comes from Plutarch *Cato Maior* 21.

²⁸ Theodor Mommsen, *Römisches Staatsrecht*, 3 (Leipzig, 1887), 509–10.

²⁹ Cicero *De Republica* 3. 16.

³⁰ *Imperialism, A Study* (London, 1938), 247–48.

³¹ On the facts see N. G. L. Hammond, *Epirus* (Oxford, 1967), 634–35.

³² Frank, *ESAR*, 1: 188.

while the Italians declined. . . . The land was held by the rich, who used slaves instead of free men on the farms.³³ As everyone in the Senate (and outside it for that matter) must have been well aware, this great supply of slaves was a direct result of Roman foreign policy.

It might be possible to argue that the moral principles or social ethos of the Roman aristocracy made it indifferent or almost indifferent to private economic gain of the kinds that I have been describing. Some such view seems to be behind a number of the arguments that have been used against economic interpretations of Roman imperialism. The moral principles and social ethos of the Roman aristocracy before the first century are obscure subjects, still in need of a comprehensive scholarly treatment. The evidence is difficult, all the more so since even in the Roman aristocracy there might be quite a variety of outlooks. The qualities that were most admired by the highest aristocracy—high birth and *virtus*, an untranslatable quality that was best exemplified in an aristocrat's military successes and holding of offices—had nothing to do with self-enrichment. But there is no good reason to believe that the second-century aristocracy despised wealth or even the acquisition of wealth, and there is not even any reason to believe that they paid little attention to the acquisition of wealth.³⁴ A recent writer on the subject referred to the "old principle 'omnis quaestus patribus indecorus visus' "³⁵ (all financial gain was thought unsuitable for senators). The phrase comes from Livy's brief description of the passing of the *Lex Claudia* in 218; it serves him as an explanation of the law, but it is obviously crude—the *Lex Claudia* was not intended to deprive senators of "all financial gain." Some, but not all, forms of financial gain were regarded as unsuitable for senators. Livy's phrase is without historical value for 218,³⁶ and for the second century as well. Some much better evidence about the attitude of the aristocracy toward the acquisition of wealth in the period of the *Lex Claudia* can be found in the funeral eulogy of L. Caecilius Metellus, *pontifex maximus* and twice consul, delivered by his son in 221. He had achieved ten great and excellent things, in obtaining which wise men spent their lives: he was an outstanding warrior, an excellent orator, and a mighty general; under his command great deeds had been done; he reached the highest honors; he had great wisdom; he was regarded as a leading senator; he acquired great wealth by honorable means (*pecuniam magnam bono modo invenire*); he left behind many children; and he enjoyed a great

³³ Appian *Civil Wars* 1. 7. 29–31.

³⁴ M. I. Finley, "Technical Innovation and Economic Progress in the Ancient World," *Economic History Review*, 18 (1965): 31, states that there were "no subconscious guilt feelings" about wealth in the ancient world; but for second-century Rome the proliferation of sumptuary laws may suggest that this generalization goes too far.

³⁵ P. A. Brunt, "The Equites in the Late Republic," *Second International Conference of Economic History* (Aix-en-Provence, 1962; Paris-The Hague, 1965), 1: 126, referring to Livy 21. 63. 4.

³⁶ Compare Adolf Lippold, *Consules, Untersuchungen zur Geschichte des römischen Konsulates von 264 bis 201 v. Chr.* (Bonn, 1963), 93–95.

reputation in the state.³⁷ These ambitions are so conventional that we should assume that "acquiring great wealth by honorable means" was conventional as well. According to some, the only honorable means by which such a man as Metellus could have acquired great wealth was inheritance,³⁸ but this is mere supposition, and *invenire* is quite the wrong verb for inheriting.

Some forms of financial gain were naturally looked down upon by second-century aristocrats: when the elder Cato stated that usury was not *honestum*, morally respectable,³⁹ he was probably expressing the general opinion of his class. Its attitude toward what Cato calls *mercatura*, or trading (he is thinking of fairly large-scale maritime trading), is not so clear: Cato's only explicit objection to *mercatura* is not that it is morally unrespectable, but that it is dangerous. The *Lex Claudia*, though it was passed without the support of the Senate, suggests that two generations earlier there was some general feeling that senators ought not to engage in some forms of large-scale trading—but even the *Lex Claudia* allowed the use of smaller ships for transporting crops to market. We should assume that, with the exception of usury and some banausic activities that are so obviously excluded that they do not need to be mentioned and with the doubtful exception of some forms of maritime trade, other means of self-enrichment were approved by second-century aristocrats. It would surely be very paradoxical if the immediate profits of war were regarded by the aristocracy as a less than honorable way of acquiring wealth. Furthermore, in order to achieve some of their other ambitions, particularly obtaining the highest political honors (on which the *virtus* of an aristocrat depended), they will have found money of great importance,⁴⁰ and it is unlikely that the income from estates long in the possession of their families was sufficient.

Did Roman aristocrats as a matter of fact enrich themselves very much by means of war and empire? That there was an immense growth of luxury is evident from contemporary evidence and needs no arguing. No second-century Roman can have possessed wealth on the scale of the fortunes of Pompey or Crassus, but such figures as we have are almost useless because they do not refer to the size of incomes. When Aemilius Paullus died in 160 he left the sum of sixty talents (1.44 million sesterces), according to Polybius, not really a very large sum; but, as Polybius himself says, Paullus was not at all well off⁴¹—indeed he cannot have been regarded as such in his own circle, since Polybius says that the sort of lavish gladiatorial games that were given to honor his death would cost as much as thirty talents.

³⁷ Pliny *Naturalis Historia* 8. 140, also printed in Enrica Malcovati, ed., *Oratorum Romanorum Fragmenta* (3d ed.; Turin, 1967), no. 6, fr. 2. On the importance of this passage see Lippold, *Consules*, 74–84.

³⁸ Compare D. C. Earl, *The Moral and Political Tradition of Rome* (London, 1967), 32.

³⁹ Cato *De Agri Cultura* preface.

⁴⁰ See H. H. Scullard, *Roman Politics, 220–150 B.C.* (Oxford, 1951), especially 23–25, on the growing importance of various forms of bribery in second-century politics.

⁴¹ Polybius 18. 35. 5; compare 31. 22.

Similarly Scipio Aemilianus was, he says, a man of moderate property, for a Roman.⁴² This at least shows us that by the standards of the Greek world Polybius considered the Romans of his time to be rich. According to Polybius, Alexander the Isian, who was the richest man in Greece, possessed a little over 200 talents, 4.8 million sesterces in Roman terms. It is likely that there were a number of Roman fortunes of this size by Polybius' time.

Now it may be the case that in spite of all this the Roman aristocracy did not allow any wish for economic gain, either public or private, to impinge on those decisions that concerned war. If the views set out above are correct, it must certainly have been quite difficult for them to prevent such considerations from having some effect, all the more so because military success was so assured. Wars always had their dangers of course, but Roman arms were virtually invincible and everyone knew it, particularly after the battle of Pydna. If one survived, self-enrichment was almost automatic.

Roman foreign policy in the second century was, within certain limits of prudence, generally aggressive and interventionist. As has already been mentioned, there were certain other restraints besides prudence, and they had their effect, mainly in relations with the Greek world before the Third Macedonian War (171–167). The aggressive and interventionist character of Roman policy has not always been clearly perceived, partly because of failure to make the distinction, introduced into ancient history by Rostovtzeff and recently emphasized by Badian, between hegemonial and annexationist imperialism—it is generally believed that there was a certain added caution about annexation.⁴³ (Annexation was not in any case necessarily the most rewarding method of economic exploitation.) Various factors can be seen to have contributed to this aggressiveness and interventionism: the desire of aristocrats for the psychological and political rewards of military success itself, an irrational degree of fear concerning some foreign states, and probably a constant tendency to believe, as well as to state, that the defense of the Empire required the obedience of foreigners to Rome's will in matters that were really of slight or no importance for Roman security. Some distinctions are difficult to make—the desire for the glory of celebrating a triumph might merge into the desire for economic benefits. I know of only one case in which an ancient writer states it as his own view that a second-century Roman general made war for the sake of plunder—characteristically in Spain, where, according to Appian, L. Licinius Lucullus, the consul of 151, attacked a tribe “out of a desire for glory and out of a need for money.” Appian says that Lucullus was poor, but he seems to mention these motives not because they were unusual, but in order to explain the fact that Lucullus made his attack illegally, without the authority of the Senate.⁴⁴ Since it is

⁴² Polybius 18. 35. 10. For the correct interpretation of this passage, see F. W. Walbank, *A Historical Commentary on Polybius*, 2 (Oxford, 1967): 597.

⁴³ For this distinction see M. I. Rostovtzeff, *The Social and Economic History of the Hellenistic World* (Oxford, 1941), 70–71; and Badian, *Roman Imperialism*, 4.

⁴⁴ Appian *Spanish Wars* 51.

plain that senators must have recognized the economic advantages to the state and to themselves of the foreign policy that was pursued, and since we have no reason to think that they regarded such things as unimportant—quite the reverse—economic motives must be taken to have had a considerable effect. Economic exploitation was not confined to activities that were the incidental side-effects of military victory, as is most clearly shown by the enslavement of the one hundred and fifty thousand Epirotes in 167 and by a similar act of enslavement carried out on a vast scale in Sardinia in 177.⁴⁵ For reasons that have been explained, the presence of economic motives is not excluded by the fact that the stated reasons for war were of a quite different kind. It can be added that if everyone in the Senate recognized the economic advantages of the general policy, they did not have to be debated at length. Of course there may have been some senators who were utterly indifferent to private economic gain. (Were there any who were indifferent to public economic gain?) I am not denying the existence of, or even attributing secondary importance to, motives that can be classified as “political.” Perhaps the best formulation that is possible on the surviving evidence is to say that desire for economic gain was an important factor predisposing senators to take aggressive and interventionist decisions in foreign policy, and there is no reason to doubt that at least on some occasions it played a more immediate part in leading to such decisions.

LITTLE HAS BEEN SAID so far about the private economic interests of those outside the Senate. Nonsenatorial businessmen were obviously not indifferent to the economic advantages of war. The sort of businessmen who could expect to receive contracts like the ones allotted by a praetor in 169 for six thousand togas and thirty thousand tunics for the army in Macedon were probably sympathetic to the policies that led to there being a Roman army in Macedon. And there certainly were some occasions when Roman businessmen benefited directly from Rome’s ability to have her will obeyed in more and more parts of the world, and from the willingness of Roman governments to secure privileges for businessmen, such as the freedom from port dues obtained for Roman citizens and Latins from Ambracia in 187 and the free harbor established at Delos in 167. (Both Romans and non-Roman Italians benefited from this, and the latter have for a long time been supposed to have been much more numerous than the former among Delian businessmen, but some recent research tends to reverse this conclusion.)⁴⁶ From 123 the contracts for the collection of taxes in the province of Asia were given out by the censors at Rome, and Roman businessmen profited considerably. It may be the case that the Jugurthine War was imposed on a

⁴⁵ Livy 41. 28. 7–9.

⁴⁶ A. J. N. Wilson, *Emigration from Italy in the Republican Age of Rome* (Manchester, 1966), 105–11.

reluctant Senate by business interests. But in general it can be agreed that nonsenatorial businessmen were not powerful enough in the second century to get particular decisions about foreign policy from the Senate, and they were certainly not powerful enough to impose a whole outlook on the Senate. They failed to get many of the economic privileges that they would presumably have liked, in particular the right to collect the taxes of Sicily, which remained in the hands of Sicilians, and those of Spain, where the taxes were collected by the quaestors. It has been suggested that Roman business interests were not in fact happy to see the commercial cities of Corinth and Carthage destroyed in 146;⁴⁷ by our lights, that would have been the rational reaction. Insofar as businessmen in general did have any influence on such matters, its total effect was surely in favor of aggressive and interventionist policies, but Tenney Frank and his followers were correct to play down the influence of nonsenatorial business interests on foreign policy.

One other group of Romans needs to be considered, in addition to senators and nonsenatorial businessmen, namely the body of those Roman citizens who were liable for military service. It is not at all fashionable to suppose that there was much exercise of popular sovereignty at Rome in the second century, least of all in foreign affairs. There was, however, one way in which the popular will must have had some effect from time to time, and that was in regard to war. There was a recurrent and increasing difficulty in recruiting soldiers for the Roman army, and this difficulty eventually became a serious crisis. The reduction of the minimum census of the fifth class from eleven thousand to four thousand *asses*, an action designed to increase the number of citizens eligible for military service, which was probably taken at some date between 200 and 150, and the eventual "proletarianization" of the Roman army by Marius in 107 were consequences of this problem. The nature of these difficulties is, one may suspect, somewhat veiled by the sources; Livy preferred to spend time retelling a story, no doubt very pleasing to his readers, about the centurion Spurius Ligustinus, who in 171 set an example to his fellow ex-centurions by doing his duty and agreeing to serve at whatever rank was assigned to him. That was how Roman soldiers were meant to behave. It seems to have become particularly difficult to find sufficient recruits for the Third Macedonian War. The Spanish wars of 153–133 intensified the problem to such an extent that, even from our very scanty sources for the period, we know that in 151 and in 138 the consuls were imprisoned by the tribunes of the plebs (whose responsibility it was to protect citizens from the unjust operation of the levy), and the government was forced to ease the conditions of military service. Solving this problem was probably among the aims of Tiberius Gracchus' radical program in 133. We do not, it is true, have any specific evidence that the Senate's

⁴⁷ For example, by Cassola, *I gruppi politici romani*, 55–56; and Badian, *Roman Imperialism*, 20–21.

deliberations were influenced by all this, but the nature of the sources makes that unsurprising.

What determined the attitudes of potential soldiers toward war and toward particular wars? It was a duty to serve, and that might have been that. There will naturally have been plenty of enthusiasm for defeating such obvious national enemies as Hannibal—not that many of Rome's enemies in the second century fell into this category. But we are not dealing with 1914 Europeans, ignorant of war and therefore eager for it. It is clear that the prospects of danger and discomfort will often have been weighed against the prospect of booty, and that when armies had been raised the most common threat to discipline was conflict over booty. We can get a view of an authentically nonaristocratic attitude toward war from Plautus. When the slave Epidicus needs to raise money quickly, he says: “ego de re argentiaria / iam senatum convocabo in corde consiliarium, / quoi potissimum indicatur bellum, unde argentum auferam.”⁴⁸ (I shall now summon a meeting of my mental Senate to take counsel on financial questions, against whom it is best to declare war, and where to steal some money.) Comic poets are notoriously treacherous material for historians, but this is not merely a translation from Plautus' Greek original, as the concepts and terminology show,⁴⁹ and it seems reasonable to draw from it the inference that war was regarded (as well it might be) as a means of making money. Thus we can add some further economic motives to the more effective ones that have already been attributed to the Senate.

To conclude: while no attempt should be made to revive “mercantilist” explanations of Roman imperialism in the second century, some economic motives can be detected. Combinations and conjectures can be fatal flaws in this type of explanation, but given the nature of the evidence, most of which is apologetic and, except for Polybius, far removed in date from the events, some combinations and conjectures are justified.

⁴⁸ Plautus *Epidicus* 158–60.

⁴⁹ Compare Eduard Fraenkel, *Plautinisches im Plautus* (Berlin, 1922), 234; E. J. Bickerman, “Notes sur Polybe, III. Initia belli Macedonici,” *Revue des Etudes Grecques*, 66 (1953): 482.

Christian-Islamic Confrontation in the West: The Thirteenth-Century Dream of Conversion

ROBERT I. BURNS, S.J.

CRUSADER SPOKESMEN ENTERTAINED some hope of converting enemy Muslims, though for long their concern remained minimal. War hysteria, especially during the early crusades, hardly provided the propitious atmosphere. Christians, steeped in generations of propaganda against a relentless foe, made poor messengers of the Gospel. Some priests proved so hostile toward Muslims as to argue against their conversion. Custom and expediency combined, in any case, to dispose of conquered populations by establishing their religion and social order as a kind of privileged enclave, while unconquered regions militantly opposed any infiltration of Christianity outside their own similar enclaves. As the twelfth century wore on, dissent against armed crusading grew,¹ the *chansons de geste* echoed with references to Muslims converted singly and en masse,² and the first stirrings of a literary and intellectual rapprochement with Islam made themselves felt.³ True, dissent did not

This paper was first delivered as a general address at Brown University in February 1970, while I was a visiting professor in the James Chair there.

¹ P. A. Throop, *Criticism of the Crusade: A Study of Public Opinion and Crusade Propaganda* (Amsterdam, 1940), ch. 5. Throop deals largely with the twelfth century.

² Heroes of the *chansons* commonly fell in love with beautiful Moorish maidens, "insisted they become converts before marriage," refrained from kissing the mouth before their beloved's conversion, and had the sacraments of baptism and marriage conferred together. See A. Robert Harden, "The Element of Love in the *Chansons de Geste*," *Annuaire Mediaevale [Duquesne Studies]*, 5 (1964): 63, 73-74, 77-79. Allan Cutler challenges the common assumption that the First Crusade was entirely divorced from any idea of conversion or mission; see his "The First Crusade and the Idea of Conversion," *Muslim World*, 58 (1968): 57-71, 155-64. James Waltz repudiates the concept of prethirteenth-century missions to Muslims in his "Historical Perspectives on 'Early Missions' to Muslims: A Response to Allan Cutler," *ibid.*, 61 (1971): 170-86.

³ For background to the intellectual and mission confrontation, see Norman Daniel, *Islam and the West: The Making of an Image* (Edinburgh, 1962), though concerned largely with a later period; Aldobrandini Malvezzi, *L'Islamismo e la cultura europea* (Florence, 1956), ch. 4; Ugo Monneret de Villard, *Lo studio dell'Islām in Europa nel xii e nel xiii secolo*, Studi e testi, no. 10 (Vatican City, 1944); J. W. Sweetman, *Islam and Christian Theology: A Study of the Interpretation of Theological Ideas in the Two Religions* (London, 1954-67), pt. 2, vol. 1, ch. 4; R. W. Southern, *Western Views of Islam in the Middle Ages* (Cambridge, Mass., 1962); James Kritizek, *Peter the Venerable and Islam* (Princeton, 1964); Juan Vernet, "El conocimiento del Islam por la cristiandad del occidente a través de los cantares de gesta," *Boletín de la real academia de buenas letras de Barcelona*, 31 (1965-66): 351-54; and M. T. d'Alverny, "La connaissance de l'Islam en occident du ix^e au milieu du xii^e siècle," in *L'Occidente e l'Islam nell'alto medioevo*, Settimane di studio del centro italiano di studi sull'alto medioevo (Spoleto, 1965), 577-603. See also two works specializing respectively on the work of the Dominicans and the

slow the military machine or its essential popular support, and the early study of Islam yielded little more than a closet literature for parochial Christian intellectuals. A new orientation nevertheless emerged, gathering strength especially in the western Mediterranean where political and military circumstances favored for a time the Christian side of the crusade–Holy War balance. In the thirteenth century “the overall strategy of Christendom underwent modification”; the battle now was “not only military but doctrinal, through a dialogue of controversy.”⁴

Nowhere does this spirit of combative dialogue appear more visibly than in the realms of Arago-Catalonia, which subdued and attached to itself in the thirteenth century an Islamic kingdom roughly the size of the crusader kingdom of Jerusalem during its earlier, palmy days.⁵ Thrusting into a sophisticated commercial region belonging to Islam’s heartland, the Aragonese crusade had persisted stubbornly for almost a quarter century. Preceded by an abortive foray in 1225, its central victories fell between 1232 and 1245. In 1243 Murcia, the Islamic principality south of Valencia, had affiliated itself with Castile as a tributary state under colonial garrison. Murcia’s actual conquest, largely by Aragon’s armies, came only in 1266.⁶ For nearly forty years after the Valencian crusade a series of revolts and crises threatened to loosen Christendom’s grip on the kingdom of Valencia and demonstrated that region’s residually Islamic character.

The Christian fever for converting Muslims can be charted, in its ups and downs, both in precrusade and crusader Valencia. Important in itself as a little world of Muslims ripe for the harvest, the Valencian kingdom served

Franciscans—Berthold Altaner, *Die Dominikanermissionen des 13. Jahrhunderts, Forschungen zur Geschichte des kirchlichen Unions und der Mohammedaner- und Heidenmission des Mittelalters* (Habelschwerdt, 1924); and Ramón Sugranyes de Franch, *Raymond Lulle, docteur des missions, avec un choix de textes traduits et annotés* (Schöneck-Beckenried, 1954).

⁴ M. D. Chenu, “Les ‘gentils’ au xiii^e siècle,” in his *Introduction à l’étude de Saint Thomas d’Aquin* (Paris, 1950), introd. This new orientation may be related to the wider dream for conversion in India, Tartary, and China; see for example the long discussions by Roger Bacon of the five great religions, including Buddhism, and how to win their adherents by persuasion. *Opus maius*, tr. R. B. Burke (Philadelphia, 1928), vol. 2, pt. 7, sec. 4, pp. 787 ff. See also Christopher Dawson, ed., *The Mongol Mission: Narratives and Letters of the Franciscan Missionaries in Mongolia and China in the Thirteenth and Fourteenth Centuries* (New York, 1955), xxiii ff., 193–94, 220. An elaborate bibliography of thirteenth-century mission history is supplied by Hans Wolter in Hubert Jedin *et al.*, *Handbuch der Kirchengeschichte* (Freiburg, 1963–), vol. 3, half-vol. 2, pp. 273–76.

⁵ Palestine from Tyre and Dan in the north to below Gaza and Beersheba is 150 miles, and from above Beirut to Gaza some 180 miles; the width varies from 28 miles (Acre to the Sea of Galilee) to 55 miles (Gaza to the Dead Sea), widening very much of course beyond the Jordan. In comparison, from Valencia’s Vinaroz to Biar is 150 miles; from Valencia’s northern border down to Murcia city, conquered by James I, is 180 miles; the width varies from 40 to 60 miles, both measurements typical. In the thirteenth century the Christian Holy Land had shrunk to a fragment of its former extent. On the Valencian kingdom and the crusade, see Robert I. Burns, *The Crusader Kingdom of Valencia: Reconstruction on a Thirteenth-Century Frontier* (Cambridge, Mass., 1967), 1: 1–15; 2: 370–76, with chronology on 307.

⁶ Juan Torres Fontes explores this episode in depth, correcting previous scholars, although missing evidence of the conversion theme, in his *La reconquista de Murcia en 1266 por Jaime I de Aragón* (Murcia, 1967). See also his “Los mudéjares murcianos en el siglo xiii,” *Murgetana* [Murcia], 17 (1961): 57–90. Subject to the corrections by Torres Fontes, see also Antonio Ballesteros y Beretta, *Alfonso X el Sabio* (Madrid, 1963), ch. 9.

also as a beachhead or gate through which the proselytizers sought to penetrate North Africa under the aegis of Aragon's king. Islamic Spain and North Africa lay open to some such penetration, the Almohad unity of the area forever fragmented into mutually suspicious or inimical principalities since the disasters that followed Islam's defeat at Las Navas in 1212. The most important of these fragments for a time was Ifriqiya—roughly Tunisia and part of Algeria—whose Hafsîd dynasty after mid-century appropriated the awesome title of caliph. Christian powers, especially Aragon, enjoyed a range of entry into the North African states. Christian merchants crowded Tunisian and Moroccan ports, sometimes residing in mercantile enclaves; Christian nobles, townsmen, and even borrowed army groups served the sultans' battlefields; and a succession of ransomers, slavers, diplomats, and other visitors lent their varied presence. A reverse flow carried Muslim merchants, diplomats, ransomers, Berber troops on loan to the Christian king, and immigrant farmers and artisans invited by king, Church, nobles, and townsmen. This swirl of movement offered chances to influence or proselytize, though always with discretion and on sufferance. Franciscans set up a Morocco mission in 1219. In 1232 the Dominican bishop of Morocco found martyrdom, as did the Franciscans at Ceuta in 1227. This *ecclesia marrochitana* was by intention missionary rather than a chaplain adjunct to the Christian merchant and military groups, and it roused opposition from the merchants.

On the diplomatic level contacts between North Africa and Christian states evolved in a variety of patterns, occasionally assuming more universal significance. In the power struggle between empire and papacy Tunis became a center for the wavering Hohenstaufen cause, a place where Aragonese agents helped nurse it back to strength. The career of Ibn Sab'în illustrates in its own way such wider dimensions. From North Africa he influenced the Islamic rapprochement with Frederick II and Louis IX; "representing in those years the conscience of Islam," after the fall of Baghdad in 1258, he worked in the East for a truce with Christendom so as to confront the Mongols while preventing the imminent recapture of Constantinople by the Byzantines.⁷ The Muslims even sent an embassy to the papal court to have the pope force Castile to respect its provisions of the Alcaraz treaty concerning Murcia.⁸ Similarly in 1259 the pope intervened for Tunis, as ally of Aragon, against the crusading fleet of Benedict of Rocafort, archbishop of Tarragona.⁹ Political and commercial interchange served to underline the continuing influence upon Europe of Islamic literature, philosophy, and learning. Poets and wan-

⁷ Louis Massignon, "Ibn Sab'în et la 'conspiration hallâgienne' en Andalousie, et en orient au xiii^e siècle," in Emilio García Gómez et al., eds., *Études d'orientalisme dédiées à la mémoire de Lévi-Provençal* (Paris, 1962), 2: 674-75.

⁸ Torres Fontes, *La reconquista de Murcia*, 73-74. See also Francisco Fernández y González, *Estado social y político de los mudejares de Castilla: considerados en sí mismos y respecto de la civilización española* (Madrid, 1866), 104.

⁹ See the protest of King James I to the archbishop of Tarragona, doc. of Oct. 1, 1257, on the "piraticam tirannidem," in Joaquín Miret y Sans, *Itinerari de Jaume I "el Conqueridor"* (Barcelona, 1918), 263.

dering musicians broadcast the literary wares of Islam throughout Spain and into other lands. The brilliant contributions of Toledo's twelfth-century school of translators, reflected in lesser centers of translation like Tarazona, continued into the thirteenth century, when Alfonso X made his large additions to this treasury.¹⁰

YEARS BEFORE SUMMONING the First Crusade, on the occasion of restoring the primatial dignity to captured Toledo in 1088, Pope Urban II urged upon its incumbent archbishop, Bernard, a policy of conversion. "With warm affection we exhort you, reverend brother, that you live worthy of so high and honored a pontificate, taking care always not to give offense to Christians or to Muslims; strive by word and example, God helping, to convert the infidels to the faith."¹¹ Gregory VII, leader of the Gregorian reform, anticipated Urban's program by dispatching a mission to Spanish Muslims in 1074, hoping to convert the ruler of Zaragoza, Aḥmad I al-Muqtadir.¹² These eleventh-century efforts may be related to the wider work of French Cluniac monks in Spain. They found an echo in limited but significant efforts at conversion during the First Crusade.

Hope for Islam's conversion, from eleventh-century stirrings, took on strength by the middle of the twelfth century. The first translation of the Koran in any language, sponsored by Peter the Venerable and completed by a team of collaborators about 1141, found echo in the translation of Mark of Toledo in 1213 under the patronage of Toledo's great archbishop, Roderick Jiménez of Rada. Later King Peter III of Aragon commissioned two separate translations into Catalan.¹³ Pope Lucius III, promulgating regulations for

¹⁰ J. E. Keller sums the king's contributions in his study of Alfonso as literary man, *Alfonso X, el Sabio* (New York, 1967), ch. 8. Keller's work supplements Ballesteros' monumental biography and supplants Evelyn S. Proctor's view of King Alfonso as patron as presented in *Alfonso X of Castile, Patron of Literature and Learning* (Oxford, 1951).

¹¹ Pope Urban II to Bernard, Anagni, Oct. 15, 1088, in Demetrio Mansilla Reoyo, ed., *La documentación pontificia hasta Inocencio III, 965-1216* (Rome, 1955), doc. 27, p. 44. There existed, of course, a contrary school of canonical thought that increasingly stressed the crusades as the continuation of Rome's struggle against the barbarians and saw the infidel as belonging by rights under Christian colonial control. Michel Villey, "L'idée de la croisade chez les juristes du moyen âge," in *X Congresso internazionale di scienze storiche, Relazioni* (Florence, 1955), 3: 572-74; and his *La croisade, essai sur la formation d'une théorie juridique* (Paris, 1942).

¹² Allan Cutler argues that Abbot Hugh of Cluny was behind this mission. "Who Was the 'Monk of France' and When Did He Write?" *Al-Andalus*, 28 (1963): 249-69. See too the documentation by D. M. Dunlop in "A Christian Mission to Muslim Spain in the Eleventh Century," *Al-Andalus*, 17 (1952): 259-310. Abū Ja'far al-Kharrāji, in his history of the caliphs, rails at the "low, dissolute, and contemptible Muslims" joining the Cid at the end of the eleventh century, "most" of whom converted to Christianity. Ahmad b. Muhammad al-Maqqari, *The History of the Mohammedan Dynasties in Spain*, tr. Pascual de Gayangos (London, 1840-43), 2: app., p. xxxix.

¹³ On the beginnings of serious missionary study of Islam, see Kritzeck, *Peter the Venerable and Islam*, including a translation of the *Summay*, which was meant for Christians (p. 116); on the Cluniac connection see pages 20, 64. See too Marie-Thérèse d'Alverny, "Deux traductions latines du Coran au moyen âge," *Archives d'histoire doctrinale et littéraire du moyen âge*, 16 (1947-48): 69-131. The Catalan translations were entrusted to Guillermo Ses Comas and, through Ferrer Gilabert, to Francis Pons Zaclota. Antonio Rubió y Lluch, ed., *Documents per l'història de la cultura catalana mig-eva* (Barcelona, 1908-21), 1: docs. 269, 313, 334.

the crusading Order of Santiago in 1184, sounded the same note as Urban a century before: he “stringently commanded that they aim at one thing alone in their fighting against the Saracens—not love of worldly praise, not desire of shedding blood, not greed over land acquisition—but either that they defend Christians from their onslaught or else that they may be able to draw these [Saracens] to practice the Christian faith.”¹⁴ Less than a decade later, in 1192, Pope Celestine III asked the Toledo archbishop to dispatch a bilingual missionary for preaching to Christians in “Morocco, Seville, and other cities of the Saracens.”¹⁵ Presumably such a missionary also served the dreams of conversion.

In the twelfth century Peter the Venerable had urged winning the Saracens “not as our people [so] often do, by weapons, not by force but by reason, not by hate but by love.”¹⁶ An even more prestigious contemporary, St. Bernard of Clairvaux, the organ voice of Christendom, had urged that energy be channeled into converting Muslims and “masses of pagans” instead of trying to influence Europe’s Jews, who were protected by a providential time lock—the Jews “have their set time [for conversion], which cannot be advanced.”¹⁷ At the opening of the thirteenth century the great Pope Innocent III, inclining to a popular belief that the world might end around 1284, envisioned a final crusade effort in East and West to prepare the mass conversion both of Jews and Muslims.¹⁸ Jacques de Vitry, preaching and baptizing converts in the crusader Holy Land, informed Europe from 1217 to 1221 that “many” Muslims, “if they heard sound doctrine, would easily be converted”; “many” already had their children baptized out of superstitious hope for health; “frequently a number of Saracens cross over” into Christianity, only to return to “the

¹⁴ Pope Lucius III to the Knights of Calatrava, Verona, Nov. 17, 1184, in Mansilla Reoyo, *Documentación pontificia*, doc. 124, p. 149. Converts from Islam were settled as military colonists in some of the border regions by crusaders in the Near East. G. E. von Grunbaum, *Medieval Islam: A Study in Cultural Orientation* (Chicago, 1947), 58.

¹⁵ Pope Celestine III to Martin López of Pisuerga, archbishop of Toledo, June 4, 1192, ed. by Fidel Fita in “Noticias,” *Boletín de la real academia de la historia*, 11 (1887, pt. 2): 456. The letter is quoted in Demetrio Mansilla Reoyo, *Iglesia castellano-leonesa y curia romana en los tiempos del rey San Fernando, estudio documental sacado de los registros vaticanos* (Madrid, 1945), 75. Perhaps the “Marrachios” of the document stands, by scribal miscopy, not for Morocco but for Marrakesh, to balance Seville.

¹⁶ Kritzeck, *Peter the Venerable and Islam*, 161. See also Sweetman, *Islam and Christian Theology*, pt. 2, vol. 1, pp. 69–87.

¹⁷ St. Bernard *De consideratione, ad Eugenium papam* 1. 1. 3 (ed. Jean Leclercq et al., *Opera* [Rome, 1957–68], 3: 433). See also S. W. Baron, *Social and Religious History of the Jews* (2d ed. rev.; New York, 1952–), 4: 300. The vulgarized and malicious echo of this tradition appears in Alfonso X’s chapter on the Jews in his *Siete partidas*, where he stresses that Jews live as strangers in Christian society, providentially serving as a perpetual reminder that their forebears had crucified Christ.

¹⁸ Southern observes that, with one surprising exception in Pope Innocent III, the apocalyptic role of Islam had no influence “on the main current of responsible thought,” although it did influence the popular and general spirit. *Western Views of Islam*, 42. On Innocent’s chiliasm, see the theory of Allan Cutler in “The Ninth-Century Spanish Martyrs’ Movement and the Origins of Western Christian Missions to the Muslims,” *Muslim World*, 55 (1965): 337–39. Cutler also develops the theme in “Why Did Pope Innocent III Want the Fourth Lateran Council (1215) to Impose Distinguishing Clothing on Jews and Muslims?” Paper read at the third biennial Conference on Medieval Studies, Mar. 1966, Western Michigan University.

customary uncleanness of the pagans" because Christian life was too demanding. But as the end of the world neared, he claimed, the sword would consume the recalcitrant while the other Muslims would convert.¹⁹

In the 1260s Roger Bacon hungrily eyed the multitudes of Muslims, Mongols, Buddhists, and pagans ripe for conversion, and especially their philosophers, specifically including among them the Muslims living in the midst of the Christians of Spain. Crusading alone was ineffective against Muslims and must be supplemented by an army of learned men; these "men wise in all knowledge" should preach by persuasion. At the far turn of the century a student of Aquinas, Pierre Dubois, concocted an elaborate program for the conversion of the infidels.²⁰ By this time contacts with the Mongol world had widened the range of hope for conversion—merchants were traveling regularly into China; the convert from Peking, Raban Sauma, had given communion to Pope Nicholas IV and Edward I of England; and John of Montecorvino, after a tour of baptizing through India, had founded his diocese at Peking. Meanwhile the theme of the baptized Saracen haunted Europe. In folk literature his archetype was Renouard, a convert-crusader of heroic legend, whom Dante places among the epic warriors in Paradise; Marco Polo's opening chapter spins a long tale revealing the "secret" conversion of the caliph of Baghdad and "many" followers.²¹ For North Africa the dream waxed strongest in the middle of the thirteenth century and again at the century's end; then it waned and forever died.

Thirteenth-century crusading popes like Honorius III, Gregory IX, and Innocent IV encouraged conversion of Muslims by persuasion; from time to time the program tended to focus on a promising princely candidate. Internal dislocations rendered plausible the widespread Joachimite belief marking 1260 as the beginning of the millennium; as part of his vision Joachim of Flora had persuaded many that Islam, the seventh head of the beast, would be conquered less by fighting than by preaching.²² The noble 'Abd al-'Aziz,

¹⁹ Jacques de Vitry to William Du Pont des Arches *et al.* at the University of Paris, undated, written in two parts in late 1216 and early 1217; and de Vitry to Pope Honorius [Apr. 18, 1221], in R. B. C. Huygens, ed., *Lettres de Jacques de Vitry (1160/1170-1240) évêque de Saint-Jean-d'Acre, édition critique* (Leiden, 1960), 83, 87-89, 94, 97, 137, 152.

²⁰ Bacon, *Opus maius*, pt. 3, ch. 14, p. 112. In the *Opus tertium* (1267) Bacon sets as targets the "philosophi infideles, Arabes, Hebraei, et Graeci, qui habitant inter Christianos, ut in Hispania et in Aegypto et in partibus Orientis." *Opera quaedam hactenus inedita*, ed. J. S. Brewer, in no. 15 of *Rerum britannicarum medii aevi scriptores*, or *Chronicles and Memorials of Great Britain and Ireland during the Middle Ages* (London, 1858-96), 295. Pierre Dubois' program is a recurrent theme in his *The Recovery of the Holy Land*, tr. W. I. Brandt (New York, 1956). In the 1270s at Acre William of Tripoli undertook to refute the Koran so as to undercut Muslim resistance and bypass even persuasive preaching: "et sic simplici sermone Dei sine philosophicis argumentis, sine militantibus armis, sicut oves simplices petunt baptismum Christi et transeunt in ovile Dei." Jacques Quetif and Jacques Échard, *Scriptores ordinis praedicatorum recensiti* (1719-23; facsimile ed., Paris, 1959), 1: 265.

²¹ Dante, *Paradiso*, canto 18, line 47; Marco Polo, *Travels*, ch. 1.

²² E. R. Daniel disentangles the chiliastic Joachimite thread from collateral conversion themes; exgetical rather than prophetic, this branch of Joachimism saw Islam as ordained by God, to be met by the faith and prayers of the Christian remnant; from 1268 the infidel masses would convert, leaving only fringe peoples as the army of Antichrist, with the Franciscans as Joachim's monastic preachers to the Gentiles and Jews. Arnold of Vilanova, physician to Peter III of

described by Emperor Frederick II as a relative (*nepos*) of the Tunis sultan, created a sensation when he arrived at Rome to be baptized.²³ Gregory IX entertained hopes for converting the sultan of Tunis, much as he hoped to convert the sly sultan of Damascus and the Almohad caliph Abu 'l-*Alā*. St. Louis, king of France, directed his crusade toward Tunis partly because contemporaries believed in the possibility of winning the current Hafsīd ruler. At mid-century Raymond of Penyafort informed Rome that North Africans were inclining toward the faith, "especially the politically powerful and even the king of Tunis." After the last Almohad fled from Morocco to the court of James I, bearing along a gaggle of relatives and retainers, to become pensioners of the Crown of Aragon at Calatayud and Valencia, some of these relatives became Christians. In 1285, as the duel projected at Bordeaux between Charles of Anjou and Peter III of Aragon captured the imagination of many European knights, who vied for a place in the lists, a son of the sultan of Morocco asked to accompany the party of Aragon; he promised to turn Christian if Peter overcame the Angevin.²⁴

It is likely that King Peter's namesake who appears in a document as "Peter of Tunis, son of the sultan of Tunis," was the son of the dethroned caliph Abū Ishāq Ibrāhīm and the godson-protégé of King Peter.²⁵ Five years after the last Valencian revolt the sultan of Bougie, Ibn al-Wazīr, offered to become a Christian as well as Peter's "man and his godson and his vassal." The king shared this good news with Rome.²⁶ At the end of the century the Hafsīd caliph's cousin and counselor, Abū Yaḥyā Zakariyā I, al-Liḥyānī, conveyed the impression that he was about to convert; he continued these maneuvers after he became sultan in 1311, confiding how he was a Christian at heart and had postponed baptism only from motives of prudence.²⁷ To Spanish kings, complacent in the winning of men like Don

Aragon and to his sons, helped elaborate this view. Daniel, "Apocalyptic Conversion: The Joachite Alternative to the Crusaders," *Traditio*, 25 (1969): 129-30, 138, 143-44. On the chronology of Joachite chiliasm, see Norman Cohn, *The Pursuit of the Millennium* (New York, 1961), 99 ff.

²³ Altaner, *Dominikanermissionen*, 108.

²⁴ The report of Raymond of Penyafort to the master-general of the Dominicans survives only in a resumé, reproduced in J. M. Coll, "Escuelas de lenguas orientales en los siglos xiii-xiv," *Analecta sacra tarraconensia*, 17 (1944): app. 2, p. 138 (Coll's article continues in 18 [1945]: 59-90; and 19 [1946]: 217-40); the report's date of 1246 is corrected to 1256 or 1258 by Altaner in *Dominikanermissionen*, 90-91n. The other episodes and documentation are conveniently gathered in Charles Dufourcq, *L'Espagne catalane et le Maghrib, au xiii^e et xiv^e siècles, de la bataille de Las Navas de Tolosa (1212) à l'avènement du sultan mérinide Abou-l-Hasan (1313)* (Paris, 1966), 98, 120, 188, 205, 248, 251, 260-70, 285. Ibn Khaldūn also records the conversions of the Almohad exiles.

²⁵ Dufourcq, *L'Espagne catalane*, 270. See the similar episode in Ibn Khaldūn, *Histoire des Berbères et des dynasties musulmanes de l'Afrique septentrionale*, tr. William MacGuckin, Baron de Slane; rev. ed. by Paul Casanova and Henri Pérès (Paris, 1925-56), 2: 403.

²⁶ Raymond Muntaner, *Crònica*, ed. Enrique Bagué (Barcelona, 1927-52), ch. 44; see also ch. 52. Jerónimo de Zurita confuses him with the brother of the king of Tunis; he dates the episode in 1281. *Anales de la corona de Aragón* (Zaragoza, 1610-21), vol. 1, ch. 13. Ibn Khaldūn notes that Muslims denounced him to Tunis as irreligious.

²⁷ See Ibn Khaldūn, *Histoire des Berbères*, 2: 439-40; Angeles Masiá de Ros, *La corona de Aragón y los estados del norte de África, política de Jaime II y Alfonso IV en Egipto, Ifríquia y*

Ferdinand, son of the emir of Baeza, and Abū Zayd, Islamic potentates seemed likely targets for the lightnings of grace.²⁸ Abū Zayd made a particularly fine display because he was not only *wālī* over Valencia and eastern Spain but a prince of the blood, descended from the great Almohad founder.

Of Abū Zayd's several sons, Muslim and Christian, one presented an adventurous horizon for the dreamers of conversion. This son was "Zeit Aazon," a Latinized name probably standing for "the *sayyid* al-Ḥasan." Older authors speak of Abū Zayd's having two sons baptized at Murcia in 1241, with St. Ferdinand of Castile and Prince Alfonso present as godfathers, under the baptismal names Ferdinand and Alfonso, the latter eventually becoming a knight of Santiago. The son Ferdinand poses no problem and may have received his baptism at Murcia under some such conditions; Abū Zayd had briefly been a vassal of King Ferdinand, after all, and is said to have accompanied him to the Seville crusade. Alfonso, on the other hand, seems to be the al-Ḥasan who appears abruptly in a papal bull of 1245, an adult Muslim preparing for baptism and transferring to the Santiago Order his "kingdom of Zala" in Africa. Innocent IV approved the transaction, with its expressed hopes of basing there an easy conversion of North Africa. The puzzle of this grandly named principality can be clarified by reference to a parallel case in the early fourteenth century, the pseudoconversion of a "king of Africa" who proves to have been merely governor for the Tunisian town of Mahdia. In converting, the son of Abū Zayd was probably conveying title to the coveted Morocco port of Salé, against which Alfonso X launched a crusade in the late 1250s.²⁹ Allied with such projects for princely baptism was the conviction that mass conversion would issue concomitantly. This had been a pattern in converting large areas of non-Islamic paganism that now lay safely within Christendom. King Peter III of Aragon told the pope that by extending the crusade into the region later called Algeria "the greater part [of the population] will become Chris-

Tremecén (Barcelona, 1951), 56–58, 236–37; Dufourcq, *L'Espagne catalane*, 413, 488–94. Al-Lihyānī's successor-pretender claimed a vision of the Virgin Mary, according to a report sent to the pope in 1325.

²⁸ Don Ferdinand Abdelman, convert son of Ibn Muḥammad, the ruler of Baeza, lived in the Seville *morería* as spokesman and protector for its Muslims. Antonio Ballesteros y Beretta, *Sevilla en el siglo xiii* (Madrid, 1913), 101. Roque Chabás y Lloréns covers the career of Abū Zayd in his "Çeit Abu Çeit," *El archivo*, 5 (1891): 143–66, 288–304, 362–76 and continued in *ibid.*, 6 (1892): 407–09. Even the leader of Valencia's revolt or countercrusade, al-Azraq, came to King James "and told me he wanted to become a Christian," suggesting marriage to a relative of the powerful lord Carroz, admiral of Catalonia. James I, *Crònica (Llibre dels feyts)*, ed. J. M. de Casacuberta (Barcelona, 1926–62), ch. 375.

²⁹ For the proposal by "Zeit Aazon rex Zale ilustris" to transfer title, see the bull of Pope Innocent IV confirming the donation of this kingdom and granting faculties to establish churches exempt from episcopal control, Sept. 24, 1245, in José López Argüeta and Antonio Aguado de Córdoba, eds., *Bullarium equestris ordinis militiae Sancti Iacobi de Spatha* (Madrid, 1719), 166. It is more conveniently copied by Chabás y Lloréns with Spanish translation in "Çeit Abu Çeit," 6 (1892): 408–09. The identification as a son of Abū Zayd is extrinsic to the papal text. Al-Ḥasan is the most obvious but not the only possible reconstruction of the name; the Zeit prefix, also used for other sons, is probably the same Almohad honorific held by the father—*sayyid*. On the Mahdia "kingdom of Africa" and on the Salé crusade, see Dufourcq, *L'Espagne catalane*, 493, 24–25; and Dufourcq, "Un projet castillan du xiii^e siècle: la 'croisade d'Afrique,'" *Revue d'histoire et de civilisation du Maghreb*, 1 (1966): 26–51. See also Ballesteros, *Alfonso X*, ch. 7.

tians.”³⁰ Four local “chiefs and lords,” describing themselves as “blood relatives” of the Tunisian king, swore to Peter that “fifty thousand persons—men, women, and children” would welcome baptism.³¹

The obsession also gripped Valencian crusaders. King James took the cross hoping “to convert” the Muslims of Valencia to Christianity.³² A highly symbolic act, performed under the walls of besieged Valencia city, was the public baptism of Muslim converts. To set legal precedents for a court test over metropolitan ownership of the proposed Valencian diocese, the bishop of Albarracín, under armed guard and with economy of movement, improvised an ordination, a confirmation, and a grant of indulgence; though no baptismal font stood at hand, the essential concern with conversion was signified when the bishop baptized two local Muslims in St. Vincent’s Church just outside the enemy’s walls.³³ Not long after this crusade Penyafort could report to the pope from Barcelona on the great success of his Dominican convert makers in Spain. The groundwork was laid, the door stood open, and now only a sufficiency of clerics was needed to bring in the souls. With Raymond Lull, he believed that the accumulating sheaves proved the harvest was ripe.³⁴ Rational dialogue and disinterested love would conquer all.

The convert movement in the conquered kingdom of Valencia has left ample traces in surviving documentation—individuals, colonies of converts, neo-Christians as a social problem and as a class, organized programs of conversion, baronial resistance to the movement, and even a guild or confraternity of converts. Elsewhere I have itemized their numbers and explored the dimensions of the movement, including the preaching program;³⁵ and in a separate study I have demonstrated the lively prejudice felt by local Christians toward convert and Muslim alike.³⁶ The methodology of the movement remains to be examined, particularly those approaches that gave the movement its tone and

³⁰ Muntaner, *Crònica*, ch. 52. The region here is Coll or western Ifriqiya.

³¹ *Ibid.*, ch. 85. On contemporary prophecies about the conversion of the Muslim world, see Throop, *Criticism of the Crusade*, 134–35.

³² “Intentionem quam habebat ad pessundandum sarracenicam nationem et eam ad fidem convertere crucifixi, ut eius nomen actolleretur.” Antonio Ubieto Arteta, ed., *Crònica de San Juan de la Peña, version latina* (Valencia, 1961), 148; also in Tomás Ximénez de Embún, ed., *Historia de la corona de Aragón (la más antigua de que se tiene noticia) conocida generalmente con el nombre de crónica de San Juan de la Peña* (Zaragoza, 1876), 151.

³³ “Ordinatio ecclesiae valentinae,” fol. 145, in José Sanchís y Sivera, ed., *La diócesis valentina* (Valencia, 1920–21), 2: 362; also in Roque Chabás y Lloréns, *Episcopologio valentina* (Valencia, 1909), 393. A critical edition of the “Ordinatio” by Vicente Castell Maiques is ready for the press, part of his larger work on the ecclesiastical geography of Valencia. The witness was John Dominic of Teruel. The details are not correlated, but apparently the baptisms took place among the other jurisdictional activities at St. Vincent’s at that time.

³⁴ Penyafort report, in Coll, “Escuelas de lenguas orientales,” 17 (1944): 138. See the correction of the date of this document in note 24 above.

³⁵ “Journey from Islam, Incipient Cultural Transition in the Conquered Kingdom of Valencia (1240–1280),” *Speculum*, 35 (1960): 337–56. I have since discovered additional individuals in the manuscripts of the Crown Archives, notably three converts haled into court for helping sixty settlers riot and sack in Alcira’s Moorish quarter. Peter III, Reg. Canc. 39, fol. 210v, June 26, 1277, Archivo de la corona de Aragón.

³⁶ “Social Riots on the Christian-Moslem Frontier: Thirteenth-Century Valencia,” *AHR*, 66 (1960–61): 378–400.

that strike the modern student as bizarre. Facing an Islamic country, the medieval missionary had the option of five tactics: secret conversions, via commercial, chaplain, or other contacts; fanatic confrontation, designed to precipitate a dramatic response; infiltration via metaphysical dialogue with whatever Islamic savants came to hand; diplomatic maneuvers toward winning a potentate, in whose footsteps many subjects could drift into Christianity; or finally, cracking the military carapace by conquest, to expose an Islamic region to public proselytism.

IN THE USE OF ALL five methods during the thirteenth century the new mendicant orders stood to the forefront. This was logical, since they constituted the mobile and main missionary force of a Christendom grown too suddenly complex and expanded. They combined the zeal of freshly founded orders with the new learning of the universities, and because they fitted their times so aptly they drew into membership some of the most promising spirits of the day. They constituted efficient international organizations, disciplined, with high *esprit* and with novel vision and techniques. Mendicants advised on crusade projects, spurred the fighting forces to greater efforts, and planted priories when the smoke of battle cleared. They roamed the conquered countryside with programs of preaching. Within Islamic lands they contacted princes, served as chaplains to merchants or mercenary troops, sought to expand discreetly the body of native Christians forming in conjunction with such alien nuclei, and met in heady argument with Muslim sages. Taking advantage of the tolerance Muslims felt for professional holy men, they ventured to act openly at times and even to preach in public. The situation was more hopeful in the crusader East, where native Christian bodies confused the scene and Byzantine polemic had prepared some foundation. North Africa, on the other hand, entertained a special awe for the holy fanatic.³⁷

Prominent among new techniques, or techniques given a new dynamism, was the Franciscan tactic of confrontation. Outrageous, consciously ineffective, yet designed to engage the forces of heaven at some mystical level, it seized the imagination of contemporary Christendom. Much like the Christian "Martyr

³⁷ Even in the Near East, Martiniano Roncaglia concludes, the tolerance with which Muslims welcomed and heard missionaries, attested by Jacques de Vitry, applied only to crusader-held territories; elsewhere the welcome extended at best for subject-Christian enclaves of Islamic rulers. *St. Francis and the Middle East*, tr. S. A. Janto from *Storia della provincia [francescana] di Terra Santa*, 1 (3d ed.; Cairo, 1957). On the Byzantine polemic, see especially Karl Güterbock, *Der Islam in Lichte der byzantinischen Polemik* (Berlin, 1912); [Adel-]Theodore Khoury, *Les théologiens byzantins et l'Islam, textes et auteurs (viii^e-xiii^e s.), polémique byzantine contre l'Islam* (Lyons, 1966); and J. Meyendorff, "Byzantine Views of Islam," *Dumbarton Oaks Papers*, 18 (1964): 115-32. An elaborate ritual for receiving Muslim converts into the Byzantine Church in the thirteenth century, with detailed repudiations of Islam by the convert, survives as book 20 of Nicholas Choniates, *Thesaurus orthodoxae fidei* (in the Latin version, *Maxima bibliotheca veterum patrum et antiquorum scriptorum ecclesiasticorum*), ed. Marguerin de la Bigne (Lyons, 1677), 25: 186-88. On the Franciscans, especially in Aragon and Valencia, see my *Crusader Kingdom of Valencia*, 1: 198-207; 2: 467-69, with the studies cited. Sugranyes de Franch provides a bibliography on Franciscan missionary work in his *Raymond Lulle*, 53-54.

Movement" of the ninth century, which disconcerted early Spanish Islam, the new protest movement proposed to preach boldly in mosques and medinas the iniquity of Islam and the triumph of the Cross.³⁸ The preacher hoped for miracles but settled for the more probable martyrdom, and successful use of the tactic required prior commitment to death. St. Francis of Assisi made three such attempts. He proposed to convert the Almohad caliph of western Islam in 1213, but fell sick in Spain. He traveled boldly to Damietta in Egypt in this spirit in 1219, where against all probability he charmed the sultan al-Malik al-Kāmil Nāṣir ad-Dīn.³⁹ Dante exalted "the thirst for martyrdom" that drove Francis to "preach Christ in the presence of the arrogant Sultan";⁴⁰ Giotto immortalized the scene in his painting at Santa Croce in Florence. The stigmata or wounds of crucifixion that marked the saint's body, as a surrogate martyrdom, related to this obsession.⁴¹

Moved by the victory of Castilian-Aragonese armies over the Islamic world at Las Navas in 1212, St. Francis envisioned a campaign of conversion. At Islamic Seville, for example, a band of Franciscans tried to force entry into the central mosque and had to be deported. Pushing on to Morocco they twice won exile. Two Italian Franciscans, the priest John of Perugia and the lay brother Peter of Sassoferrato, penetrated Valencia around 1228, while it was still an immemorially Islamic region, to court death at the capital city of its Almohad *wālī*, Abū Zayd. Abū Zayd had them publicly executed, to the lasting edification of neighboring Aragon; his own conversion, not long after, was popularly attributed to their blood, while his field of execution became a

³⁸ E. P. Colbert examines the literature and interpretations of the early movement in his *The Martyrs of Cordoba (850-859), A Study of the Sources* (Washington, 1962). Cutler invited further probing with his "The Ninth-Century Spanish Martyrs' Movement" (pp. 321-29), eliciting a penetrating analysis by James Waltz, "The Significance of the Voluntary Martyr Movement of Ninth-Century Cordoba," *Muslim World*, 60 (1970): 143-59, 226-36. Waltz suggests that asceticism contains a logical orientation toward martyrdom, especially the extreme asceticism encouraged by Spanish circumstance. This orientation of spirit was incorporated by influential leaders, alarmed by the cultural and consequently religious erosion of Spanish Christians at this juncture, into the program then evolving toward the *respublica christiana*, so as to resist acculturation and to polarize instead around the myths and ideology emerging in Europe. A. J. Wensinck traces the main current of orthodox teaching on martyrdom and relates it both to Jewish and Islamic attitudes in his *Semietische Studiën uit de Nalatenschap* (Leiden, 1941). His chapter 5 deals with the Eastern doctrine of martyrdom, which he compares to the Islamic doctrine (pp. 90-113) and by extension to asceticism (p. 100). For Western Christendom at our period, Franciscan scholastics like Alexander of Hales and Bonaventure devote space to martyrdom under its many aspects without really clarifying this obsessive form; the same is true of Louis Gougaud's otherwise useful study "Le désir du martyre et le quasi-martyr," in his *Dévotions et pratiques ascétiques du moyen âge* (Paris, 1925), pt. 2, ch. 4. This medieval phenomenon entered modern news in the summer of 1970 when Pope Paul VI canonized four such Franciscan martyrs of the fourteenth century. Adverting to the traditional refusal to canonize in cases of provoked martyrdom, a position formalized by Benedict XIV, the pope distinguished between intended provocation and the exercise of a preaching ministry despite the inevitably consequent martyrdom.

³⁹ Roncaglia, *St. Francis and the Middle East*, 19, 26, with discussion of dating. On martyrdom in the apostolate see pp. 81 ff., and for a list of eighteen Franciscan martyrs in the Near East from 1265 to 1289 see p. 85.

⁴⁰ Dante, *Paradiso*, canto 11, lines 100-61.

⁴¹ Giulio Basetti-Sani, *Mohammed et Saint François* (Ottawa, 1959), ch. 8.

holy place.⁴² The bizarre episode marked the start of Franciscan commitment to the conversion of the Muslims in the crusader kingdom of Valencia. The Catalan Raymond Lull, central figure of the Franciscan mission to the Muslims, meant to set off for North Africa in the same spirit of martyrdom, recoiling from violent death but deliberately courting it. Lull, an eminently rational man, devoted to philosophical argument as a prime means of conversion, nevertheless viewed even his missionary schools of language and polemic as concomitantly schools of martyrdom. Legend, echoing his lifetime thirst for martyrdom, has him killed by a mob while proselytizing in North Africa.⁴³

More prominent in the long run were the Dominicans of Valencia and North Africa. A dynamic new order, fresh from successes in Albigensian Languedoc and at the universities in Europe, the Dominicans distinguished themselves in their missionary activity by a novel double tactic—the rationalist argumentation enthusiastically resounding in the university centers, combined with cultural adaptation by means of language schools. The first element, when turned against Islam, marked the culminating phase of a polemical approach that had come hesitantly to the fore in the early twelfth century under the aegis of writers like Peter the Venerable.⁴⁴ Its thirteenth-century expression, more metaphysical, bore the stamp of the Arabic philosophers who so heavily influenced the initial century of transfer of Aristotelian treasures to the West before 1240. The Dominican bishop of Antioch asked St. Thomas Aquinas for “moral and philosophical arguments, to which the Saracens give a hearing”; Aquinas’ missionary handbook of 1270, responding to this plea, rebuked the concomitant tendency to reduce life and revelation to understandable, provable categories. He admonished his colleagues that, though Muslims were open to argumentation, one could not convert by reason; philosophy served “not to prove the faith but to defend the faith.”⁴⁵ The warning was needed. A Christian rationalist, inhumanly rational, might bend his energies to force the adversary’s mind, proceeding without regard for the Muslim’s values, limitations, or insights. Such an intellectual, or verbal, con-

⁴² The Valencia martyrs are studied at length by León Amorós Payá, “Los santos mártires franciscanos B. Juan de Perusa y B. Pedro de Saxoferrato en la historia de Teruel,” *Teruel*, 15 (1956): 5–142. They also appear in Ibn ‘Idhārī al-Marrākushī (d. 1232), *Al-Bayān al-mugrib fī ḥiṣār ajbār muluk al-Andalus wa al-Magrib*, tr. Ambrosio Huici Miranda (Tetuan, 1953–54), 1: 321. See also the sources in my *Crusader Kingdom of Valencia*, 1: 198–99; 2: 467.

⁴³ Lull historiography is replete with pitfalls; he may have sought Dominican affiliation, but the supporting documentation on this point has been challenged, and we do not know precisely when he became a Franciscan tertiary. The miniatures of the celebrated Karlsruhe manuscript show Lull’s pseudomartyrdom in detailed, successive panels.

⁴⁴ The crusade conventicle or ecclesiastical meeting held by King James at Tarragona in 1235 harshly forbade any ordinary layman from disputing about the faith either publicly or privately, under pain of excommunication. *Cortes de los antiguos reinos de Aragón y de Valencia y Principado de Cataluña* (Madrid, 1896–1922), 1: 123. On the Dominicans in Aragon and Valencia, see my *Crusader Kingdom of Valencia*, 1: 202–07; 2: 469–73.

⁴⁵ Aquinas *De rationibus fidei contra Saracenos Graecos et Armenos* (ed. R. A. Verardo, O.P., *Opuscula theologica* [Rome, 1954], no. 13, chs. 1, 2, pp. 252–53).

frontation sharpened issues, emphasizing more starkly the irreconcilable points of difference between two exclusivist faiths. Limited initial success was bound eventually to harden the opposing group. Ironically, since philosophical dialogue easily becomes aggression, more could have been achieved by a deliberately nonintellectual and tolerantly pragmatic approach.

On the other hand, if the Franciscan tactic amounted to confrontation in the politico-social ambience, the Dominican approach constituted an infiltration of circles that moderns vaguely designate as the establishment. In the relatively undifferentiated, or rather interpenetrating, Islamic society, to influence a sage was to have an impact on religious, academic, governmental-administrative, and mercantile spheres. Louis Massignon has advanced the theory that in time of crisis, as tension mounts unbearably, cultivated men seek a resolution in ecumenical or unitive directions, while the common man becomes intransigent toward external and internal enemies. Massignon sees this principle working itself out during the thirteenth century both in the East and in southern Spain.⁴⁶ The idea can be nuanced to suggest that the euphoric dialogue of conversion between Christian and Muslim academics—so fruitful in Spain according to thirteenth-century Dominicans—created stiff resistance among other Muslims of the same class. The very success of such an effort undoubtedly triggered hostility, polarization at both extremes, and effective countermeasures. This may explain the bad feeling so evident in the riots in Valencia in 1275.⁴⁷

The Franciscans did not disdain the tactic of rationalist discourse or the founding of language schools to promote it. The most eminent single wielder of the method was the Franciscan Raymond Lull, troubadour turned mystic and scholastic, whose efforts in this direction merit a special word. The pen of this pre-eminent publicist of the conversion movement poured out tireless propaganda, largely directed to Christendom; 243 works have been authenticated, prose and poetry, in four languages including Arabic. On one occasion Lull requested King James II to forward from Barcelona for his use at Tunis a small library of fifteen books of his disputations with Muslims.⁴⁸ Ignorance seemed to him the main hindrance to conversion; he reported in a sermon that though he conversed widely “with eminent and scholarly Saracens,” he had never met one who grasped Christian beliefs rightly.⁴⁹ In his *Doctrina*

⁴⁶ Massignon, “Ibn Sab‘in et la ‘conspiration hallāgienne’ en Andalousie,” 660.

⁴⁷ See my “Social Riots on the Christian-Moslem Frontier,” 380–81, 385–94.

⁴⁸ See James II to Friar Romeo Ortiz, Oct. 29, 1315, in Raymond Lull, *Opera latina*, ed. Friedrich Stegmüller et al. (Palma de Mallorca, 1959–), 2: 404–05. See the editors’ summation of Lull’s moot last years in the introduction of this edition, 1: 9–18. The number of Lull’s works varies slightly according to his several catalogers; they cover a generous range of religious themes, of course, but conversion is a major preoccupation.

⁴⁹ Lull, sermon 26, in *ibid.*, 4: 103. A convenient introduction to this aspect of Lull’s thought and to its considerable bibliography is Sugranyes de Franch, *Raymond Lull*, and the chapter on Lull’s crusade ideology in José Goñi Gaztambide, *Historia de la bula de la cruzada en España* (Victoria, 1958), ch. 8. There are briefer surveys in such general works as Malvezzi, *L’Islamismo e la cultura europea*, 110–16. Dufourcq places Lull’s African conversion activities in the diplomatic context. *L’Espagne catalane*, 299–301. Sugranyes de Franch provides large

pueril Lull argued that the savants were skeptical about Mohammad, and consequently open to conversion; if the most important came over, the masses would follow.⁵⁰ Again in the novel *Blanquerna* he insisted that Muslim sages did not believe the Islamic religion about which they were so skilled and were vulnerable to friendly argumentation.⁵¹ Lull thought Muslims “nearer the Christians than any other unbelieving people,” but reluctant to engage the problem with their intellects because of the sacrifices involved in conversion. Though many refused all controversy, many others “follow arguments and love proofs”; Christians ought therefore to pursue them with logical syllogisms.⁵²

A scene from Lull’s novel *Blanquerna*, though dealing with Jews, particularly captures his fantasy of rational conversion. The holy bishop of his story, brooding over the Christian stoning of two Jews, concludes that he could play the peacemaker by reducing differences to a single religion; forthwith, he preaches and holds seminars every Saturday in the synagogue until he absorbs both Jews and problem into the Christian unity.⁵³ Fellow Franciscans like Roger Bacon shared this Lullian enthusiasm for persuasive or philosophic polemics as the prime tool of conversion. Lull himself came to this scene a bit late, since his religious visions and consequent alteration from troubadour-seneschal to mendicant-philosopher began only in 1265. As the son of a Majorcan crusader, page with the traveling court of James I in the 1240s, and seneschal to Prince James on Majorca, he brought to his task a young lifetime of observation from the highest vantage points. Abandoning his wife for the consuming labor of preaching to infidels, some say he found himself attracted initially by the Dominican style, only later assuming the status of Franciscan tertiary.

Lull’s astonishing productivity and the impact of his fascinating character have made his single Franciscan school of language more celebrated than the whole Dominican program. The school was Holy Trinity, founded by Prince James at Miramar on Majorca in 1274, endowed with five hundred gold florins annually, confirmed by Pope John XXI in 1276, and preparing continual relays of thirteen Franciscans.⁵⁴ In *Blanquerna* Lull describes at length this found-

excerpts from Lull’s works and the *Tractatus de modo convertendi infideles* (1292) in full in his *Raymond Lull*, 129–43. The context of Lull’s life and work can be seen in Erhard W. Platzeck’s panorama, *Raimund Lull, sein Leben, sein Werke, die Grundlagen seines Denkens (Prinzipienlehre)* (Rome, 1962–64).

⁵⁰ See the text of *Doctrina pueril*, with commentary, in Sugranyes de Franch, *Raymond Lulle*, 92.

⁵¹ Raymond Lull, *Blanquerna*, tr. E. A. Peers (London, 1962), ch. 43; see also ch. 86.

⁵² See the texts in E. A. Peers, *Ramon Lull, A Biography* (New York, 1929), 67, 73. The school of Averroes recognized three modes of persuasive argumentation: rhetorical—to move ordinary men of sound intelligence; dialectical—by argument and counterargument within any common frame of suppositions, as academics use; and demonstrative—proceeding without presuppositions and from first principles, such as the rare thinkers or philosophers use. See W. Montgomery Watt, “Philosophy and Theology under the Almohads,” in *Actas, Primer congreso de estudios árabes e islámicos* (Madrid, 1964), 106.

⁵³ Lull, *Blanquerna*, ch. 75.

⁵⁴ For the confirmation of the school with its relays of thirteen, see Reg. Vat. 38 (John XXI), fols. 15v–16, Oct. 16, 1276, Archivio Segreto Vaticano, Rome. See also *A Life of Ramón Lull*,

ing "in perpetuity." Proud of its success he turned up at Rome a decade later to advocate establishing five more such schools. The language apostolate intrudes into *Blanquerna* a number of times, as does Lull's larger preoccupation with Muslim conversion. In one chapter the fictional pope keeps a converted Muslim as his "scrivener of Arabic"; in another "he sent for the friars who had learned Arabic" and who had "converted the Saracen king and a great multitude of people." In other chapters four friars learn Turkish and leave for the East; two, skilled in Arabic, write to the pope, and the pope determines to resolve language differences, which lie at the bottom of wars and sects.⁵⁵

After the turn of the century the aged Raymond Lull described to the ecumenical council at Vienne how a great number of Muslims "were subject to Christians and most of all in Spain"; he outlined a program for preaching to Muslims on Friday and to Jews on Saturday. Still sanguine, Lull wanted only syllogistic arguments. With a sustained program, he claimed, "it would necessarily follow that the Jews and Saracens would come to the way of truth."⁵⁶ At least one Muslim found humor a bulwark against such an aggressive approach. Teaching at Murcia under the patronage of Alfonso the Learned, he declined the invitation to turn Christian, arguing that since he already found himself deficient in his duties toward one God, he foresaw the measure of his failure toward a Trinitarian three!⁵⁷

Another Franciscan, not a tertiary like Lull but the friar Roger Bacon, harped upon the theme of Arabic conjoined to philosophy, with special reference for the Muslims in Christian Spain. "Arabic serves little use for theological study, but for philosophy and for conversion of infidels much," he wrote; philosophy in turn was vital for preaching to the infidels. Nor did mere conversational ability avail: "many men can be found among western Christians, who know how to speak Greek or Arabic or Hebrew, but very few who know the literary structure [*orationem grammaticae ipsius*] or how to teach it—I've put many of them to the test." Despite the eminence of Bacon and Lull, Franciscan labors in this direction remain poorly documented. Their more usual

Written by an Unknown Hand about 1311, ed. E. A. Peers (London, 1927), 5, 15. Rudolf Brummer tells the Miramar story in his "Ramon Lull und das Studium des Arabischen," *Zeitschrift für romanische Philologie*, 85 (1969): 132-43. See also Armand Llinarés, "Raymond Lulle et l'Afrique," *Revue africaine*, 105 (1961): 98-116; and his "Le séjour de Raymond Lulle à Bougie (1307) et la 'Disputatio Raymundi Christiani et Hamar Saraceni,'" *Estudios lullianos*, 4 (1960): 63-72, especially Lull's arguments in Arabic with Islamic savants and with their "episcopus ille famosus in philosophia" (pp. 67, 69).

⁵⁵ Lull, *Blanquerna*, chs. 55, 84, 88, 93-95; on the Muslim mission theme see also, for example, chs. 50, 61, 76, 87, 88.

⁵⁶ Lull, at Vienne, in Ewald Müller, *Das Konzil von Vienne, 1311-1312, seine Quellen und seine Geschichte* (Münster, 1934), 696.

⁵⁷ A. S. Tritton, *Materials on Muslim Education in the Middle Ages* (London, 1957), 144-45. Tritton cites the incident as an example of Christians and Jews attending Islamic schools. He does not identify him, but this was the great Muḥammad al-Riqūṭī (see p. 1432 below) teaching in the purely secular school of Arabic surviving at Murcia; also teaching here was the convert Bernard de Arábigo.

approach was less formal and structured; their forte was a mission of heart more than of head.⁵⁸

The Dominicans, like the Franciscans, saw the Spanish and North African regions as a unified apostolate. In 1256 the master-general, Humbert of Romans, surveying progress and opportunities, hailed Spain and North Africa together as the bright spot for Muslim conversions. Like Humbert, Pope Alexander IV joined the two areas in his bull of 1256 to the Dominican provincial of Spain, giving faculties for specialists to be sent "to the lands of the Saracens of Spain [and] over the whole kingdom of Tunis"; here Spain meant Murcia and Granada.⁵⁹ The standard monograph on Dominican missionary efforts in the thirteenth century divides the universal Muslim mission into six geographical areas, two of which comprise Spain and two North Africa; the distribution reflects the emphasis given these conjoined regions.⁶⁰

St. Raymond of Penyafort was this apostolate's leading spirit. A portentous figure in thirteenth-century Europe and moral commentator for his times, the great lawyer became general of the Dominicans in 1238. Already passionately concerned with converting Muslims, he was now able to channel the energies of the young order in that direction. The central part of his life's work in the missionary movement, however, came after his resignation from the generalate; there is reason to think he resigned precisely to concentrate on the promising areas of Tunis, Murcia, and conquered Valencia. From headquarters set up at the Barcelona friary he managed a far-flung program for the conversion of Jew and Muslim in which the Valencian kingdom figured prominently. A friend and executor of his last wishes composed a brief biography recording how Penyafort attracted Muslims by his "sweet and reasonable discourse"; he begged "means for their support from kings and prelates"; Moors came to him "as to a unique refuge."⁶¹ Another colleague characterized him as "enthusiastic for spreading the faith among Saracens."⁶² The crowd of kings and nobles gathered to pay tribute at his funeral testified to the influence at his disposal during his life.⁶³

⁵⁸ Bacon, *Opus tertium*, 88, 303-04, 33. On Bacon and Spanish Muslims, see note 20 above.

⁵⁹ Pope Alexander IV, bull of June 26, 1256, in Coll, "Escuelas de lenguas orientales," 17 (1944): 136-38. There was cause for optimism elsewhere too; for example, Christian influence was strong among the Seljuks by the second half of the century. See T. T. Rice, *The Seljuks in Asia Minor* (London, 1961), 113-14.

⁶⁰ Altaner, *Dominikanermmissionen*, ch. 5. Around 1256 a new phase of the Dominican mission in Tunis began and entered a particularly promising era after the peace of 1270.

⁶¹ *Vita antiqua*, a biography attributed to Nicholas Eymeric, in Franciscus Balme et al., eds., *Raymundiana seu documenta quae pertinent ad S. Raymundi de Pennaforti vitam et scripta* (Rome, 1901), no. 11, pp. 31-32. *Vita antiqua* was published before 1351 at the very latest. This biography of Raymond of Penyafort, and the two cited in footnotes 62 and 63, may also be found in Jose Rius Serra, ed., *Diplomatario (documentos, vida antigua, crónicas, procesos antiguos)* (Barcelona, 1954). All subsequent references, however, will be to Balme's *Raymundiana*.

⁶² Gerard of Frachet, O.P. (d. 1271), in the chronicle entitled *Vitae fratrum ordinum praedicatorum* (1254), erroneously attributed to Humbert of Romans and written more than twenty years before Raymond's death, in Balme, *Raymundiana*, no. 1, p. 3.

⁶³ Peter Marsilio describes the kings, princes, and nobles at his funeral. *Vita Sancti Raymundi de Pennaforti* (1312), extract in Balme, *Raymundiana*, no. 8, pp. 13-14.

POLEMICAL PREACHING, common and programmed in Valencia, was not the only weapon in the mendicant arsenal. Equally important as a convert-making technique was the institution of schools of Arabic studies to train specialists in controversy, especially with Muslim academics. These centers applied to the Valencian and North African situation a long-standing Dominican policy. As early as 1235 the master-general, writing from Milan to all the order, called for men "prepared to learn Arabic, Hebrew, Greek, or some other outlandish language."⁶⁴ Penyafort transformed the Arabic schools into something special. They did not aim to instruct in the elements of Arabic; students undoubtedly knew the language before arriving. The mass of Dominican missionaries from whom they came learned their Arabic without counting it anything special, probably working under veteran colleagues in the Near East. Relatively few—"selected Catalan friars" plus later volunteers⁶⁵—profited from these advanced centers designed to give facility in polemical conversation. The ideal graduate was a man like Raymond Martí, described shortly after his death as "philosophus in arabico."⁶⁶

Most of the records for these schools have disappeared, though minutes from Dominican general and provincial chapters, along with references from the lives of leaders, supply some information. A converted Moor, the Dominican Michael of Benazar (Ibn Naṣr), may have created the first such philosophy and language school shortly after the fall of Majorca; if so, it soon foundered.⁶⁷ The Dominicans placed their central schools for work with western Muslims at those spots within the area that combined density of Muslim population with maximum opportunity—Tunis for a while, then Játiva, Murcia, and Valencia city, with Barcelona as the base back home. Tunis was the first center established from Barcelona. It dates from the early 1240s, at the latest from 1245. An assignment of eight friars in 1250, often mistaken as the founding, shows it in full career.⁶⁸ Apparently it died just before 1259. The provin-

⁶⁴ Humbert to the Dominican Order, 1235, in B. M. Reichert, ed., *Litterae encyclicae magistrorum ordinis praedicatorum ab anno 1233 usque ad annum 1376* (Rome, 1900), doc. 5, p. 19. André Berthier discusses the schools briefly. See his two articles, "Un maître orientaliste du xiii^e siècle: Raymond Martin O.P.," *Archivum fratrum praedicatorum*, 6 (1936): 267–311; and "Les écoles de langues orientales fondées au xiii^e siècle par les dominicains en Espagne et en Afrique," *Revue africaine*, 73 (1932): 84–102. Coll offered the first substantial study of the phenomenon in his "Escuelas de lenguas orientales." A. Cortabarría Beitia reviews the Spanish–North African scene in the first section of his "L'étude des langues au moyen âge chez les dominicains," *Mélanges de l'institut dominicain d'études orientales du Caire*, 10 (1970): 189–248. Besides locating the schools in their contemporary ambience, in this article I attempt to offer some advances and corrections. Dufourcq has a brief and too skeptical account of the Tunis school in his *L'Espagne catalane*, 106–10; see also Altaner, *Dominikanermissionen*, 91–94. On the Valencian preaching program, see my "Journey from Islam," 345–46, 352–53.

⁶⁵ "Cathalanos electos." Marsilio, *Vita Sancti Raymundi*, p. 12.

⁶⁶ *Ibid.*, see also Altaner, *Dominikanermissionen*, 78, 95n.; on Martí, see p. 1411 below.

⁶⁷ Benazar, to whose name the title Blessed is popularly prefixed, was said to be the son of the Ben Aabet (perhaps Ibn 'Abīd) who figures in King James I's memoirs as surrendering and then helping him to conquer part of Majorca peacefully. *Llibre dels feyts*, ch. 71. See Coll, "Escuelas de lenguas orientales," 17 (1944): 123; and Dufourcq, *L'Espagne catalane*, 108, 187n.

⁶⁸ Coll, "Escuelas de lenguas orientales," 17 (1944): 124.

cial chapter at Valencia city that year decreed an Arabic school for the Barcelona priory; instead the Valencian school, continuing or briefly reopened, seems to have absorbed the Tunis people. Alternatively, the Murcian school may already have taken up the slack of the Tunisian loss; it was bilingual, providing also a chair of Hebrew.

Five Dominican language schools, to all appearances, developed by the end of the thirteenth century. J. M. Coll, their keenest historian, challenges this common conclusion, arguing rather that centers appeared and died in a broken pattern defying neat analysis. Játiva was the most constant, Murcia the most celebrated. Coll certainly defers the Murcian foundation too long however, choosing 1266 on the feeble grounds that only then did the Christians conquer Murcia.⁶⁹ Actually Murcia had been a tributary of Castile with strong internal Christian influences for two decades. Penyafort recorded many Murcian conversions for this earlier period.⁷⁰ The definitive conquest by James I in 1266 undoubtedly demanded the school's reorganizing and strengthening. Penyafort himself decreed the Murcian *arabicum*, the contemporary Peter Marsilio reported, "arranging to send selected Catalan friars, who progressed to the great advantage of souls and to the honor of his nation."⁷¹ After the troubles connected with the 1275 war this establishment broke up, the Hebrew section moving to Barcelona and the Arabic to Valencia city.⁷²

A school apparently opened or flourished at Valencia city by 1250. Friar Michael moved down from Lérida that year as professor; eight other Dominicans were posted to the city and a Valencian removed to Majorca.⁷³ This school may have been merely theological, but one hears nothing further of any kind of studies here until abruptly encountering a full *arabicum* shortly after the death of King James. The Dominicans, with priories already in six cities of James's realms and shortly to open seven more, had worked at Valencia city for over a decade by the time of the notice in 1250 and had nearly completed their second complex of large church and other buildings.⁷⁴

A letter from the master-general in 1256 may help clarify the murky academic scene at the capital city. Humbert praised "the friars in the region of

⁶⁹ *Ibid.*, 132-33.

⁷⁰ Penyafort report, in *ibid.*, 138. The stages in Murcia's absorption are described by Torres Fontes, *Reconquista de Murcia*.

⁷¹ "Studia linguarum pro fratribus sui ordinis Tunicii et Murciae statuit, ad quae fratres Cathalanos electos destinari procuravit, qui in multum fructum animarum profecerunt et in suae decoratum speculum nationis." Marsilio, *Vita Sancti Raymundi*, 12.

⁷² Coll, "Escuelas de lenguas orientales," 18 (1945): 59-60; 17 (1944): 121, 134-35. Dating the move from Murcia 1279-80, Coll posits a short-lived *arabicum* at Barcelona between the Tunis loss and the Murcia creation, but this conjecture owes something to his refusal to credit a Murcian school until 1266.

⁷³ Provincial *Acta* of 1250, in [Marie Jean] Celestin Douais, ed., *Acta capitulorum provincialium ordinis Fratrum Praedicatorum, première province de Provence, province romaine, province d'Espagne, 1239-1302* (Toulouse, 1894), 612.

⁷⁴ Burns, *Crusader Kingdom of Valencia*, 1: 204-05; 2: 470. Coll's conjecture that a language school functioned at the city from 1258 to 1266 is allied to his dubious theory on the origins of the Murcian school. "Escuelas de lenguas orientales," 17 (1944): 133.

Spain who for many years already have studied in Arabic among Saracens [and] are wonderfully proficient in the language."⁷⁵ Though contemporaries often reserved the term "Spain" for Muslim-held areas, the Dominican province of Spain included the Christian countries, Valencia city not yet belonging definitively to a particular one of its eight vicariates. Assuming that Penya-fort organized the Spanish studies of this letter along the same lines as the Tunisian, since he was directing both projects from Barcelona, the most plausible conclusion is that schools existed from before mid-century at Murcia city or Valencia city or both, a conclusion that advances our knowledge of the origins of the language schools by more than a decade.

The Valencia *arabicum* may have continued unbroken from before 1250 until it emerges again into the documentation after King James's death; or if one follows Coll's theory that the Murcia school did not take up the work of the abandoned Tunis center until 1266, then his conjecture is reasonable that the Tunis men filled in at Valencia rather than Barcelona, creating a school for the period 1258–66. A third thesis is possible. The Valencian establishment may have gone to Játiva around 1260, combining with the Hebrew chair brought down from Barcelona. The Valencia city school reappears at least by 1276 or 1277, this time either replacing or consolidating with the translated Arabic chair of the Murcian school. No Dominican *acta* survive to trace the future of the schools during the subsequent five years. In 1281 John of Puigventos is discovered in charge of the Valencia city *studium arabicum*, with five friars assigned as new students—Peter of Tartary, Nadal, Martin, and from the Cordova friary John and García. The Estella chapter that year dispatched two Moorish converts to the Valencia enterprise, the friars Savior and Dominic Sancho. This activity, often carelessly cited as a founding, no more marks the beginning than does the notice of 1250, both apparently episodes in the school's mature life.⁷⁶

The Játiva school must have persisted, though no document survives for the thirty years up to 1291. It seems to have been the most continuous, and in the first decade of the next century it proved the most active. Its location owed much to Játiva's status as civil administrative center for southern Valencia, as capital of a Muslim subprincipality almost like that of Valencia or Murcia, and as retaining after the crusade a larger Muslim population and more native atmosphere than the forcibly emptied Valencia city. Of all the kingdom's main towns, Játiva kept its Islamic administrative-social structure most fully; intellectually it became the center of Mudejar Valencia. The Játiva and Valencia priories, though part of the same Dominican province, quarreled over respective jurisdictions for preaching in the new kingdom. In a

⁷⁵ Humbert to the Dominican Order, 1256, in Reichert, *Litterae encyclicae magistrorum*, doc. 8b, p. 40.

⁷⁶ Provincial *Acta* of 1250, in Douais, *Acta capitulorum*, 626; also in Coll, "Escuelas de lenguas orientales," 18 (1945): 62; 17 (1944): 135. The names were Fr. Petrus de Tarteriis, Fr. Natalis, Fr. Martinus de Serriolo, Fr. Joannes Serranum, Fr. García de Arce, and Puigventos, "qui legat eis." For more information on Puigventos, see p. 1411 below.

compromise settlement Valencia took the northern half from Morella to the Júcar River and the Játiva house got from the Júcar to Bañeres. The division reveals a natural zone of apostolate for each and a further reason for an Arabic center at Játiva.⁷⁷

At the end of the century, when the peninsula ceased to be a single Dominican province and Aragon joined Navarre as a separate province, a general chapter decreed that Játiva should no longer receive students from all over Spain. In 1302 the Aragonese provincial chapter balanced this, urging superiors to facilitate attendance by volunteer students at Játiva. The next year Friar Peter Escarramat came as teacher, while the Játiva prior got orders to "hire and maintain one Jew who is also schooled in Arabic, or some Saracen, so that he may teach there along with the said Friar Peter."⁷⁸ Shortly afterwards Queen Blanche, the wife of James II, provided a scholarship fund in her will for "the Dominican friars at Játiva studying in Hebrew and Arabic, to support them as long as the aforesaid *studium* exists—on its demise the said revenues to go to the nuns of St. Mary Magdalene's at Valencia city."⁷⁹ The proviso for transfer of the legacy indicates that reorganization of the language school system, and phasing of its independent entities into the theological program a few years later, was already in the wind.

The language centers had been Catalan in inspiration and in early personnel.⁸⁰ James the Conqueror reproached his colleague of Castile, Alfonso the Learned, for plunging into bookish pursuits while neglecting conversion of the Mudejars.⁸¹ Alfonso could not avoid involving himself in the Murcian foundation, however, since it was for Castile that King James had conquered that territory. The contemporary Nicholas Eymeric records how Penyaafort "with the help of the lord king of Castile and the lord king of Aragon saw to the establishment of a school of Arabic language" at Murcia.⁸² When Alfonso established with the utmost solemnity his famous *studium generale* or university of Arabic and Latin studies at Seville in 1254, it was by no means a missionary but an academic enterprise, springing from the king's fascination

⁷⁷ Burns, *Crusader Kingdom of Valencia*, 1: 204, 209; 2: 470, 473. King James gave the Dominicans land for the Játiva priory in 1248, but they were still projecting a house in 1285 and may not have commissioned this until after 1290—all of which tells little of the school project at Játiva. Francisco Roca Traver, citing Gaspar Escolano, believes Queen Blanche founded the house. "Un siglo de vida mudéjar en la Valencia medieval (1238-1338)," *Estudios de edad media de la corona de Aragón*, 5 (1952): 150-51.

⁷⁸ Provincial *Acta* of 1302 and 1303, in Coll, "Escuelas de lenguas orientales," 18 (1945): 77. Peter later became *Vicarius Africae*, though in 1303 he was teaching Hebrew. In 1304 the provincial chapter assigned another group of students to Játiva.

⁷⁹ Last testament of Blanche of Anjou, Aug. 18, 1308, in J. E. Martínez-Ferrando, *Jaime II de Aragón, su vida familiar* (Barcelona, 1948), 2: doc. 57, pp. 34-39. In a wide reading of Valencian wills, this is the only item of its kind I have come across; perhaps it owes something to the queen's confessor, the Dominican Andrew Albalat, who is designated here an adviser to the executors. The will foresees that the apostolate to the Mudejars might terminate, possibly by its very success, and adds "quo cessante, dominabus Sancte Marie Magdalene Valencie dentur redditus memorati."

⁸⁰ Marsilio, *Vita Sancti Raymundi*, 12.

⁸¹ See Coll, "Escuelas de lenguas orientales," 17 (1944): 119.

⁸² Eymeric, *Vita antiqua*, 32.

Three miniatures, illustrating chess moves, from the *Libro de Ajedrez* codex, a game-book produced for Alfonso x, the Learned, at Seville in 1283. Its 150 miniatures, remarkably realistic, reflect the Muslims seen by the Christian artist(s) in southern and eastern Spain, especially at Seville and Murcia. Illustrations from the manuscript collections of the Escorial library.

Below

Explaining a chess move. Note details of the turbans and robes, which in Spain suggested men of importance, the curled shoes, and the full beards that custom and Castilian law alike imposed on Mudejars.

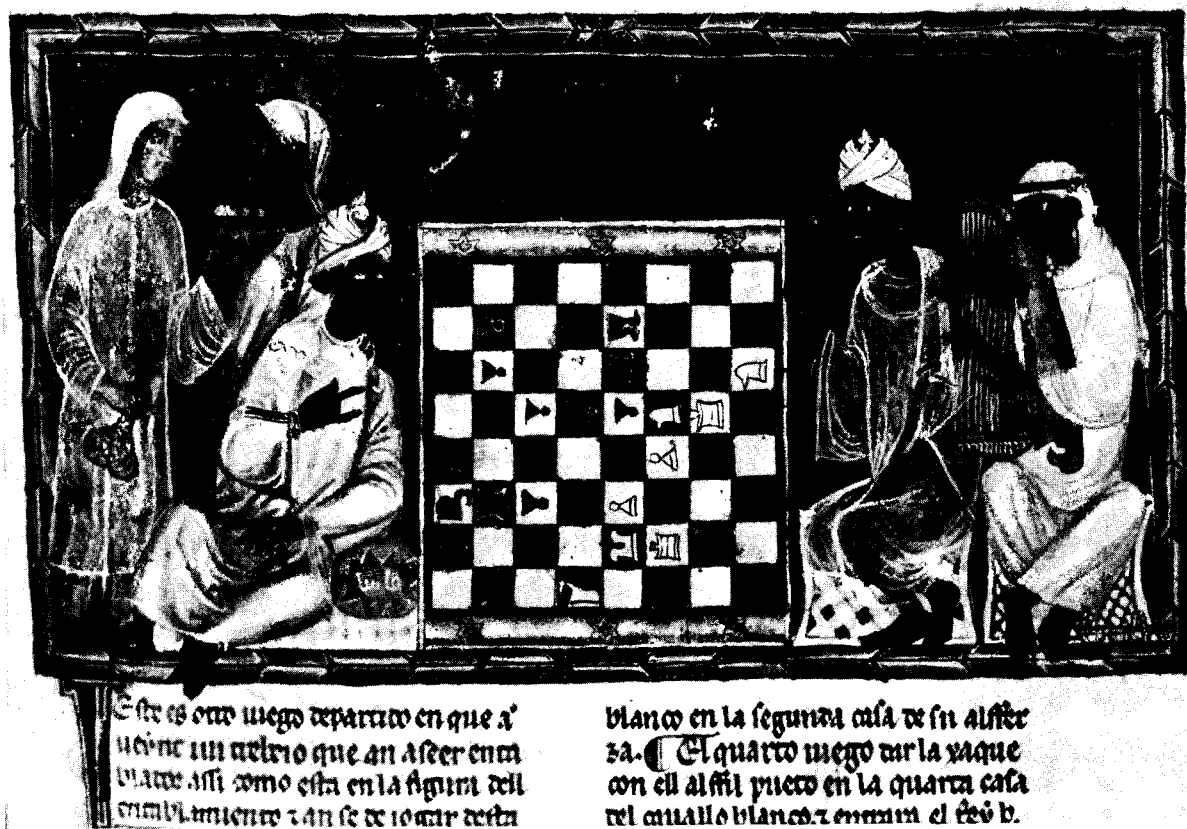
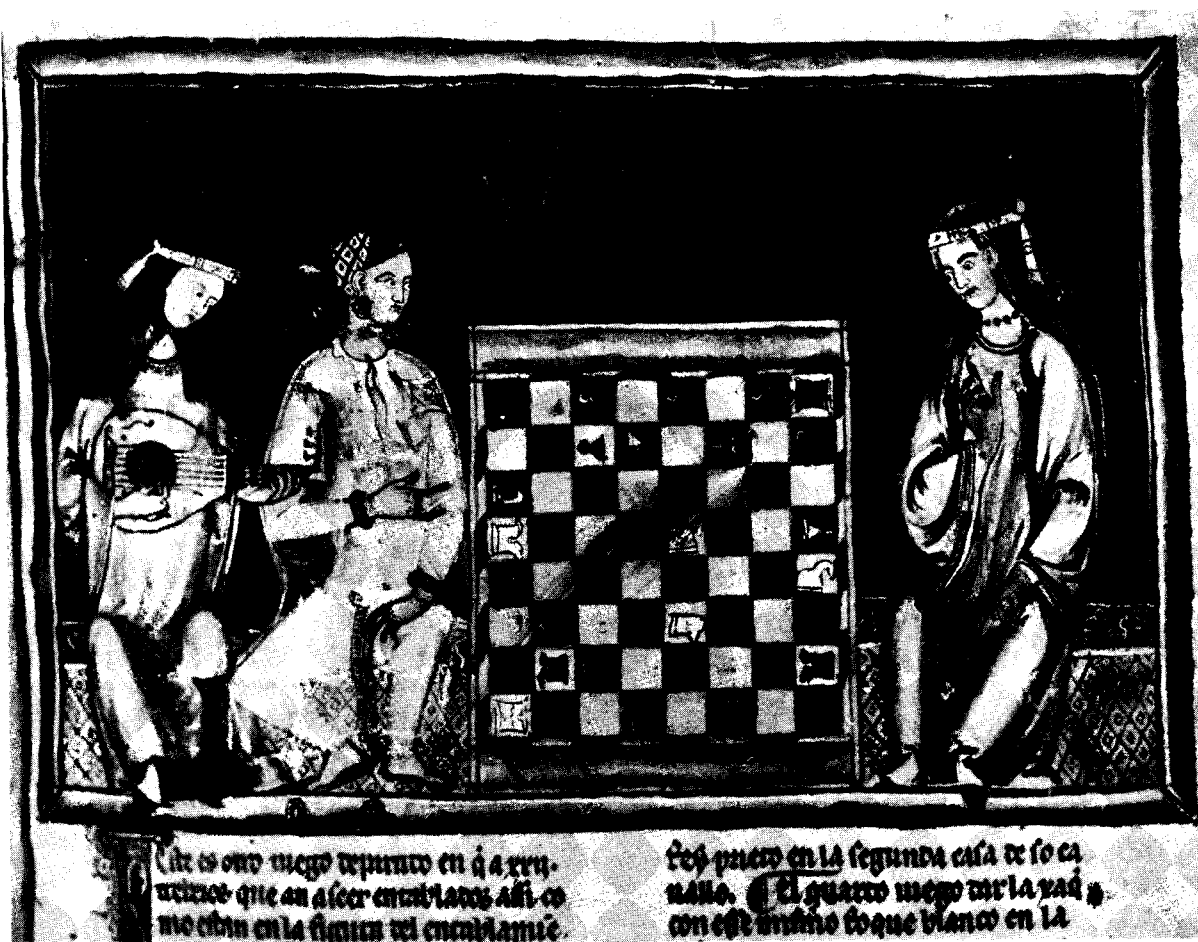
Facing page, above

Three Muslim women at chess. Note hair styles, jewelry, clothing, and musical instrument.

Facing page, below

Negro gentlemen at chess. Rachel Arié rightly dismisses Julián Ribera's suggestion that they are conventional symbols for "Moor." Most Moors in these pictures are white, and the prevailing tone of the artist is realistic; furthermore, by this time there were Christian Negroes. Note domestic utensils, veils, and musical instrument.





with Arabic culture and university learning; its charter bore the confirmatory signatures of three Islamic kings.⁸³ The language-center movement never broadened its base. Under the urging of Lull, Pope Clement v at the ecumenical council of Vienne in 1312 ordered two chairs each in Arabic, Hebrew, and Chaldean for the universities of Paris, Oxford, Bologna, and Salamanca, as well as at the papal curia, "so as to lead wanderers into the path of truth."⁸⁴ No record survives to prove that any of these began or, if they began, that they lasted long. From 1248 at least, and probably earlier, Paris boasted a college of Arabic and other Near Eastern languages that instructed relays of ten young clerics from overseas, but we know nothing about the establishment beyond that bare fact.⁸⁵

CRITICAL FOR THE SUCCESS of polemical efforts at conversion was a metaphysical theology with which to meet the Muslim savant on his home grounds, and if possible some handbooks to facilitate its deployment. The Catalan Raymond Martí composed his *Pugio fidei adversus mauros et iudaeos* for this mission.⁸⁶ With these areas in view the Catalan Penyaforç requested a similar work from Thomas Aquinas and so prompted the master work of Aquinas, the *Summa contra gentiles*.⁸⁷ Lull conceived his similarly titled *Libre del gentil* for this crusade of philosophy, probably after the disputation in 1263 with the Jews at Barcelona, but he confected other works for disputing with Muslims.

A direct or causal connection between the labors of Martí and Aquinas, argued persuasively but then abandoned on grounds of chronology, is once more being sustained. The two Dominicans had sat together as students under Albert the Great, their works display parallels and borrowings, and their separate interest in the Arago-Catalan mission seems clear. Earlier arguments by Miguel Asín y Palacios for direct borrowing by the *Contra gentiles* from the *Pugio* had been frustrated, however, by the separate works of Luís A. Getino and José M. Llovera that dated the *Pugio* at 1278, a decade after the time

⁸³ Privilege of Alfonso x, granting Seville a *studium generale* of Latin and Arabic learning, Dec. 8, 1254, in Fernández y González, *Mudejares de Castilla*, doc. 35, pp. 344-45. The confirming signatures run almost twice the length of the document proper, representing luminaries of Church and state and including twenty-nine prelates, "Don Aboadille Abenazar" the king of Granada (Abū 'Abd Allāh b. Naṣr), "Don Mahomad Abenhuc" the king of Murcia (Ibn Hūd), and "Don Abenmafot [Ibn Mahfuz], re de Niebla, vasallo del rey." See also the history and charter in C. M. Ajo G. y Sáinz de Zúñiga, *Historia de las universidades hispánicas* (Madrid, 1957-), 2: 205-07, 440-41.

⁸⁴ "Ut errantes in viam veritatis inducere . . . valeamus." *Clementinae*, lib. V, tit. i, c. 1, in Emil Friedberg and E. L. Richter, eds., *Corpus iuris canonici* (2d ed.; Leipzig, 1879-81).

⁸⁵ Working from texts in the Denifle *Chartularium*—ten "pueri" or "clerici," "tam in arabica quam in aliis linguis orientalium partium peritos"—M. M. Dufeil concludes that these must be Christians native to the Near East, of varied rites, but contemporary preoccupation with conversion suggests other hypotheses as well. "Traces d'Orient à Paris au xiii^e siècle," *Revue d'histoire et de civilisation du Maghreb*, 2 (1967): 48-49.

⁸⁶ See p. 1411 below and note 100.

⁸⁷ Marsilio, *Vita Sancti Raymundi*, 12; see also Altaner, *Dominikanermmissionen*, 94, 94n.

commonly assigned to the work of Aquinas—1261–64, or even 1258–63.⁸⁸ Evidence of a connection remained strong, so that in 1969 José Maria Casciaro fell back upon informal prepublication interchange between two friends as the explanation.⁸⁹ Meanwhile Pierre Marc, in his 1967 introduction to the critical edition of the *Contra gentiles*, grappled anew with the dating problem; external and internal evidence led him to reverse previous conclusions and to assign the work to the last years of Aquinas' life, roughly 1270–73.⁹⁰ There now seems little doubt that Aquinas used Martí's earlier *Capistrum* (1267), while Martí later drew upon the *Contra gentiles* for his own *Pugio*; further interchange, along Casciaro's line, also seems probable.

Unaware of this future revolution in dating and innocent of the complexity of Mudejar Spain, some older scholars had expressed skepticism over the missionary motives of Aquinas, seeing only the "gentiles" of the Paris academic scene as his target. Thus, M. M. Gorce rejected Marsilio's witness on the grounds that "except perhaps for some teachers or rabbis" in the realms of Aragon, "one cannot see very well what profit" could be drawn "from this work of extreme erudition and real philosophical difficulty."⁹¹ The puzzlement betrays ignorance of the high civilization flourishing in Valencian and Murcian Islam during the centuries leading up to the crusade; it also rests on a misunderstanding as to the nature of the Dominican schools for which the work was desired. Besides, Marsilio was a man eminent in the Islamic

⁸⁸ The Spanish Arabist Miguel Asín y Palacios closed a long article tracing "coincidences" between Aquinas and Averroes (as against Latin Averroism) by arguing briefly that Aquinas borrowed from Martí; his "El averroísmo teológico de Santo Tomás de Aquino" appeared in the symposium *Homenaje a D. Francisco Codera en su jubilación del profesorado, estudios de erudición oriental*, introd. by Eduardo Saavedra (Zaragoza, 1904), 271–331; see discussion of the *Pugio* question on pp. 320–23. Asín sent an offprint to Luis [Alonso] Getino, who immediately snuffed out this promising line of thought with an "open-letter" booklet, establishing a late date for much of the *Pugio*—his *La Summa contra gentiles y el Pugio fidei, carta sin sobre a Don Miguel Asín y Palacios catedrático de lengua árabe en la universidad central* (Vergara, 1905) sums up Asín's position (pp. 5–8), refutes it by chronology (pp. 8–19), poses and answers minute objections, then broadens the discussion. Asín's ghost lingered in the further corners of academe until decisively laid to rest by José M. Llovera, canon of Barcelona, in his address, "Discurso inaugural de la Sección de teología en el congreso de la Asociación para el progreso de las ciencias," delivered at Barcelona in May 1929. Llovera supplemented and scaled Getino's chronological approach; he gained wide circulation for his own ideas by allowing long quotations and paraphrases from his speech (never published in full) to appear in a major manual—Tomás and Joaquín Carreras y Artau, *Historia de la filosofía español, filosofía cristiana de los siglos xiii al xv* (Madrid, 1939), 163–66.

⁸⁹ José Maria Casciaro, *El diálogo teológico de Santo Tomás con musulmanes y judíos, el tema de la profecía y la revelación* (Madrid, 1969), 44.

⁹⁰ Marc more generally assigns the work as beginning between November 1269 and March 1270, ending between mid-December 1272 and mid-February 1273. Pierre Marc *et al.*, eds., introd. to *Liber de veritate catholicae fidei contra errores infidelium qui dicitur Contra gentiles*, 1 (Paris, 1967). Thomas Murphy sums up Marc's external and internal evidence, appraising it in the light of the previous controversy, in his "The Date and Purpose of the *Contra gentiles*," *Heythrop Journal*, 10 (1969): 405–15.

⁹¹ M. M. Gorce, "La lutte 'Contra gentiles' a Paris au xiii^e siècle," in *Mélanges Mandonnet, études d'histoire littéraire et doctrinale du moyen âge* (Paris, 1930), 1: 223–43. Père Gorce is preoccupied by the "gentiles" of Paris and the wider scope of the work Aquinas actually achieved. See also on this the discussion in M. D. Chenu, "Les 'gentils' au xiii^e siècle."

mission, distinguished in letters and learning, a counselor to James II, and well placed to know the facts. When he put his account together at the Barcelona center, thirty-five years after the death of its director Penyafort, many who had known that great man were still alive to dispute irresponsible claims. His witness is definitive. Today one can even point to the moment when Penyafort's appeal was conveyed to Aquinas—during Martí's trip to Paris, in November 1269–March 1270.⁹² Aquinas surpassed Penyafort's request for a handbook, of course, creating a master work useful against not only the Greco-Arab views in their Parisian and varied Islamic forms but against the full range of *errores infidelium* facing Christendom.

Careful survey uncovers forty students during the lifetime of King James, certainly a fraction of the total. A contemporary records that at Murcia alone "twenty friars or more of the Order of Preachers were taught in that [Arabic] language."⁹³ From study of available names, the great majority at all schools appear to have been Catalans.⁹⁴ This reflects political realities of both Aragonese Crown patronage and geography; the school areas lay under King James's control as at Valencia and Játiva, his influence as at Tunis, or his special relationship due to Catalan conquest and partial settlement as at Murcia.⁹⁵ Each group of six or more students studied for two or three years at a center, receiving at the end a kind of university degree, the *licentia disputandi* required for formal public controversy and issued by the Dominican provincial. Friars could return for a refresher course. A school might be an *arabicum* alone as at Valencia, or bilingual as at Murcia and Játiva. Bilingual *studia* could be coequal or, as apparently happened at Játiva when the Jewish controversies threw emphasis upon the Hebrew faculty, a *primarius* in the one language could control the school while a subteacher of the other language functioned under him. The pattern has confused some moderns, causing them to see a founding at Játiva when the Lérida chapter in 1312 "establishes a *studium arabicum*," the prior to provide a professor. Actually this represents an administrative upgrading of the subordinate faculty.⁹⁶

The *studia* required libraries, always an expense to collect. An indication of how the precious books were acquired comes from the testament of Raymond Deçpont, the last bishop of Valencia in the thirteenth century: as benefactor of the local Dominicans he left them a large amount of money, a scholarship at the University of Paris, and for the Játiva center sets of both *Summae* of Aquinas.⁹⁷ The majority of language instructors were not Dominicans or even converts but Muslims, especially during the early decades. A life of Penyafort by a near-contemporary says that "many of these, particularly the

⁹² Murphy, "Date and Purpose of the *Contra gentiles*," 409–10.

⁹³ Marsilio, *Vita Sancti Raymundi*, 12.

⁹⁴ Coll, "Escuelas de lenguas orientales," 17 (1944): 130.

⁹⁵ For the role of protonationalism in thirteenth-century Aragon, see my *Crusader Kingdom of Valencia*, 1: 254–56; 2: 496. On the heavier Catalan settlement in Castilian Murcia, see Juan Torres Fontes, ed., *Repartimiento de Murcia* (Murcia, 1960), viii–ix.

⁹⁶ Provincial *Acta* of 1312, in Coll, "Escuelas de lenguas orientales," 18 (1945): 79–80.

⁹⁷ Burns, *Crusader Kingdom of Valencia*, 1: 206.

erudite, inclined to accept the truth of the Catholic faith, and the teachers of the brothers in the Arabic language were almost all converted by the industry of these [Dominican] students."⁹⁸

Three Dominicans stand out in the apostolate of polemical schools in Valencia. Raymond of Penyafort loomed as the moving force until his death in 1275. His junior contemporary, the saintly John of Puigventos, Crown overseer for the wider popular mission to the kingdom of Valencia's Mudejars, received the habit at Valencia city's priory and probably took his Arabic training at Murcia. He taught at the Valencia city school, where according to a document of 1281 he held the Arabic chair, and he served as subprior of the local Dominican residence. His extensive work among *aljamas* of the Valencia kingdom caused many to regard him as a saint. He died in 1301.⁹⁹ Closer in age to Penyafort was the famed "philosophical Arabist" Raymond Martí, "beloved intimate" not only of James I of Aragon and Louis IX of France, Marsilio records, but also "of the good king of Tunis."¹⁰⁰ From Subirats near Barcelona, Martí entered the order during the opening years of the Valencia crusade and was one of eight assigned to strengthen the Tunis school in 1250. In 1256 he composed an *Explanation of the Creed* for work with Jews and Moors. His *Summa against the Errors of the Koran* in 1260 culminated the Muslim phase of his work and provided the Dominican schools with a solid text. A Hebrew student, he was diverted to this new field by public controversy with the Jews of Barcelona in 1263. In 1264, as one of a trio appointed by King James, he examined Jewish books; in 1267 he composed a polemical work for use with Jews along much the same lines as the Muslim polemic. Aside from a brief mission to Tunis in 1268–69, which won him an appearance in King James's memoirs,¹⁰¹ Martí continued this specialization. In 1278 he wrote his celebrated *Pugio fidei*, whose first section was directed to Muslims, and in 1281 took the Hebrew chair at the Barcelona *studium*. He died around 1285.

Men of lesser stature, such as the teacher Escarramat, played their roles. It is probable that friars like Sancho Boleya, ambassador to Tunis in 1299 and polemicist, involved themselves with the Mudejars of Valencia. Francis Cendra, active in the *Tunis arabicum*, as a diplomat in Africa, and as prior of the Barcelona house, must have shown interest in the sister centers. Blessed Peter

⁹⁸ Eymeric, *Vita antiqua*, 32.

⁹⁹ On Puigventos see Francisco Diago, *Historia de la provincia de Aragón de la orden de predicadores* (Barcelona, 1599), chs. 47, 48; Baltasar Sorió, *De viris illustribus provinciae Aragoniae ordinis praedicatorum*, ed. J. M. de Garganta Fábrega (Valencia, 1950), 48; Gaspar Escolano, *Décadas de la historia de la insigne y coronada ciudad y reino de Valencia* (1610–11), ed. J. B. Perales (Valencia, 1878–80), vol. 2, bk. 10, ch. 1. Sorió (d. 1557) incorrectly has Puigventos (d. 1301) die in 1320.

¹⁰⁰ The career of Martí is covered in André Berthier, "Un maître orientaliste du xiii^e siècle: Raymond Martin O.P.," 267–311; the description by Humbert of Romans is on p. 273. See also Marsilio's *Vita Sancti Raymundi* with comments by Altaner in *Dominikanermissionen*, 95n.; and Alberto Collell Costa, *Escritores dominicos del principado de Cataluña* (Barcelona, 1965), 171–72.

¹⁰¹ James I, *Llibre dels feyts*, ch. 490.

of Cadiret, another product of the Tunis school, became by 1257 inquisitor for the realms of Aragon including Valencia. Bernard of Bach held vicariate powers over the Catalan-Valencian-Murcian region from 1275. Bernard of Peregrí from Lérida, student at the Valencia *arabicum* from 1269, and later Crown inquisitor, fostered the Játiva center; he died in 1309. Most of this elite band, however, remain unknown as individuals; a handful appear as bare names receiving assignments.

The approach of the schools marks a trend away from older concern with the dogmatics of Islam. Metaphysical dialogue comes to the fore as much as theological confrontation. In short, metaphysics constituted a specialized apostolate concerned rather with the ranks of Islam's erudite. The broad front represented by compulsory attendance at rationalist preaching was only indirectly connected with the schools' apostolate to the intellectual elite. Specialized teaching undoubtedly filtered down to the ordinary theological student or friar in the Valencian kingdom's priories, but it supplemented background and tactic rather than providing the content of sermons. A double movement of conversion, in which the dramatic concept of the schools overshadows the mendicants' wider, quiet work, progressed in a parallel manner. Acquisition of conversational vulgar Arabic, perhaps with the aid of vocabulary handbooks like that by Raymond Martí,¹⁰² was as easy for the young friar as for the merchant or adventuring knight. Because of confusion between the two apostolates, Dominican success on Majorca caused Coll to postulate an early Dominican school there.¹⁰³ Under both guises the unremitting crusade of polemics, continuing far beyond the crusade of the sword, had a measure of success.

From one point of view, therefore, the schools had a limited focus of attention, unrepresentative of wider missionary work; from another angle they comprised the most formidable direct attack on Valencian Islam. The acculturative effect of converts from this class upon the Muslim community had to be traumatic. Academic Spanish Islam, heir of Averroes (Ibn Rushd) and Avempace (Ibn Bājjā), wrestled with the sons of Albert, Aquinas, and all the array of thirteenth-century scholastic genius. At this level the mendicant effort appears in a more favorable light than it does as the arrogant rationalism it became when more widely diffused. Enthusiasts for philosophy saw

[Text resumes p. 1432]

¹⁰² The Arabic-Latin lexicon or word list attributed to Martí—*Vocabulista in arabico, pubblicato per la prima volta sopra un codice della biblioteca riccardiana di Firenze*, ed. Celestino Schiaparelli (Florence, 1871)—later debated and given other attributions, was probably done by Martí in Valencia, glossed by contemporaries elsewhere, and preserved in an amplified, late thirteenth-century copy made at Majorca. Manuel Sanchís Guarner, *Els parlars romànics de València i Mallorca anteriors a la reconquesta* (2d ed. rev.; Valencia, 1961), 135–40.

¹⁰³ Coll, "Escuelas de lenguas orientales," 17 (1944): 123. Throughout this paper I have avoided the moot question of Valencia's languages, a subject explored at length in my forthcoming *Islam Under the Crusaders*; it now appears that local Muslims were not bilingual (Arabic-Romance) but spoke a vulgar Arabic plus the classical form mastered by the educated.

THE *CANTIGAS*, OR “SONGS IN PRAISE OF ST. MARY”

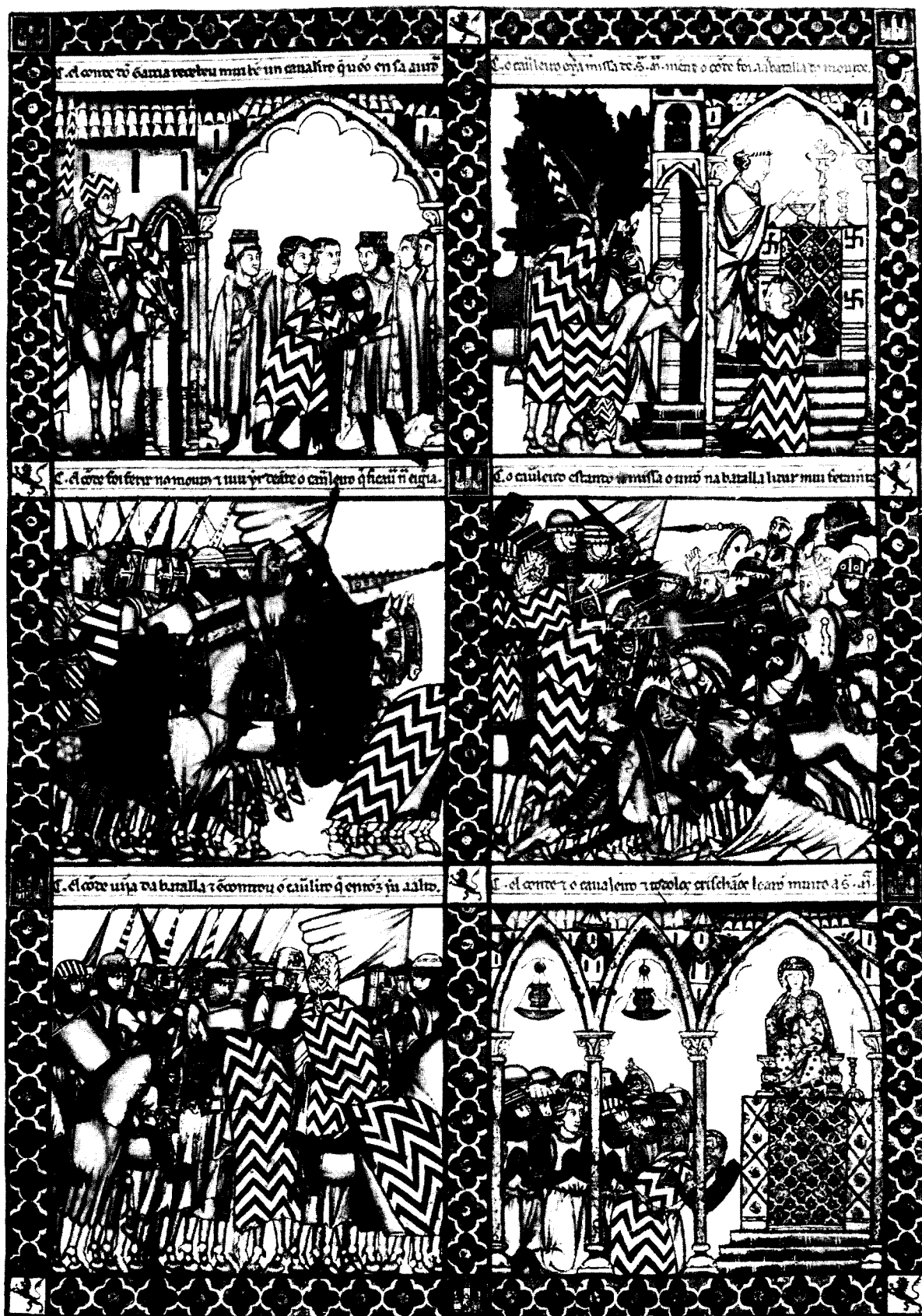
The *Cántigas*, or “Songs in Praise of St. Mary,” constructed under the personal direction of King Alfonso X, the Learned, of Castile, ranks with the contemporary masterpieces of Aquinas and Dante. Like a Gothic cathedral it interweaves a corpus of troubadour poetry to Our Lady, centering upon immemorial Miracle legends common to Christendom, with a corpus of troubadour music, including Arabic airs and rhythms—all illustrated by a heroic prodigality of 1,262 miniatures. The earliest edition dates from “after 1255”; ours, at the Escorial, is from “after 1259.”

Done by various artists at cities like Murcia, Seville, and Toledo, each episode of six panels (followed left to right like a comic strip) blazes with color. A mirror held up to daily life in the thirteenth century, the pictures comprise as well “an esthetic Bible, encyclopedically condensing all the elements of medieval art” Marcelino Menéndez-Pelayo). Our present portfolio isolates the principal Moorish and Mudejar episodes, reflecting the costumes, weaponry, attitudes, mores, and acculturative details abundantly visible around the artists. Realism overrides convention, so that scholars are able to document medieval artifacts and architecture from these miniatures. The Negroes are not conventions but a realistically portrayed stratum, which yielded many conversions; those at Valencia city later established a Christian guild or brotherhood. The paintings capture the simplistic piety that could anticipate mass conversion, while conveying the juxtaposition of two cultures in its climate of violence.

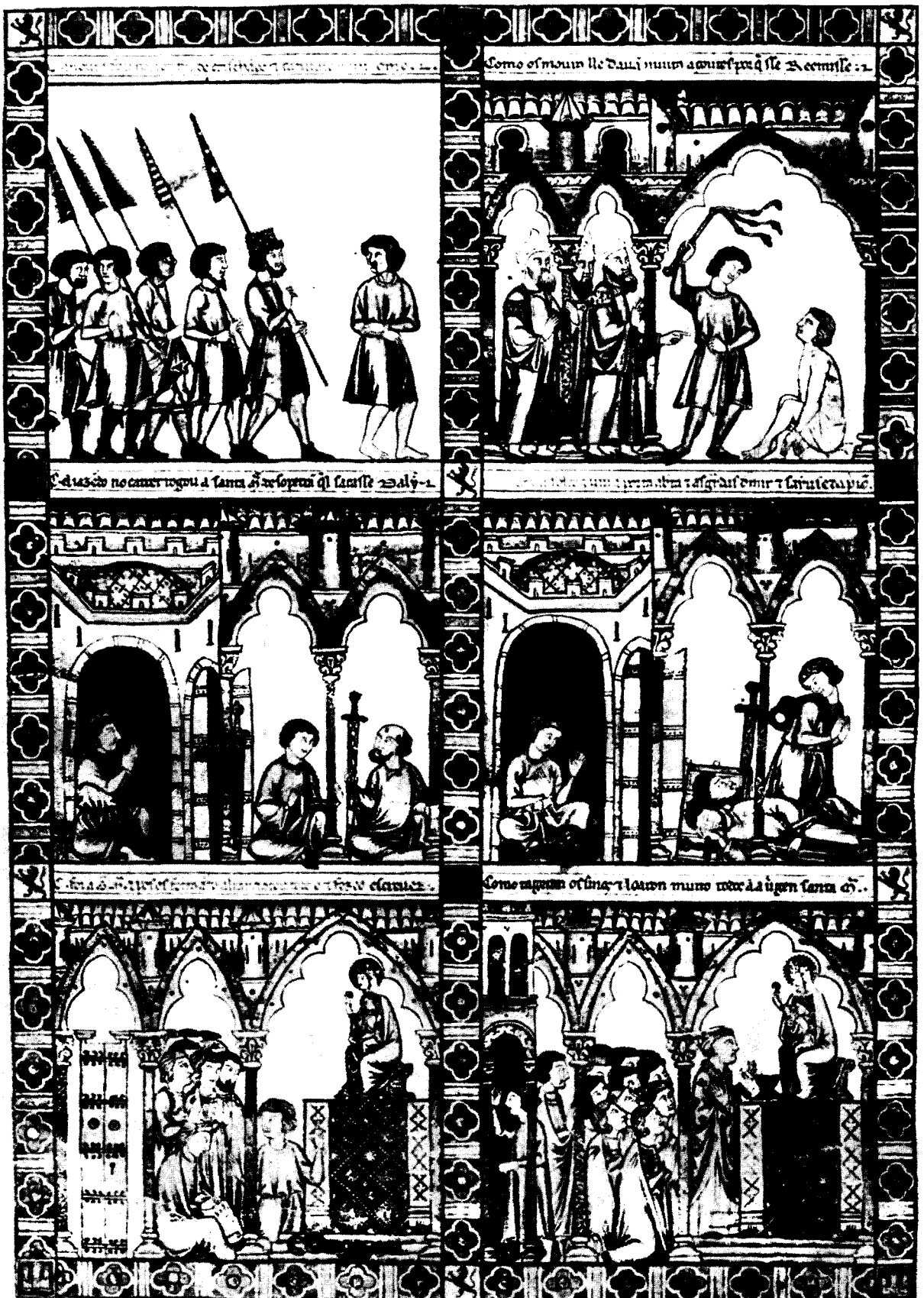
The pictures are unpublished, but José Guerrero Lovillo, in his *Las Cántigas, estudio arqueológico de sus miniaturas* (Madrid, 1949), appended all of them in black and white. Higinio Anglés edited the music in his *La música de las Cántigas de Santa María* (Barcelona, 1958–64). Bits of the music are recorded, especially on the prize-winning first album in the “Colección de música antigua española.” The poems were edited by Leopoldo A. de Cueto, Marqués de Valmar, *Cántigas de Santa María de Don Alfonso el Sabio* (Madrid, 1889), but more exhaustively by Walter Mettman, *Alfonso X, o Sábio, Cántigas de Santa María* (Coimbra, 1959–64). The illustrations printed here are reproduced from the manuscript collections of the Escorial library.



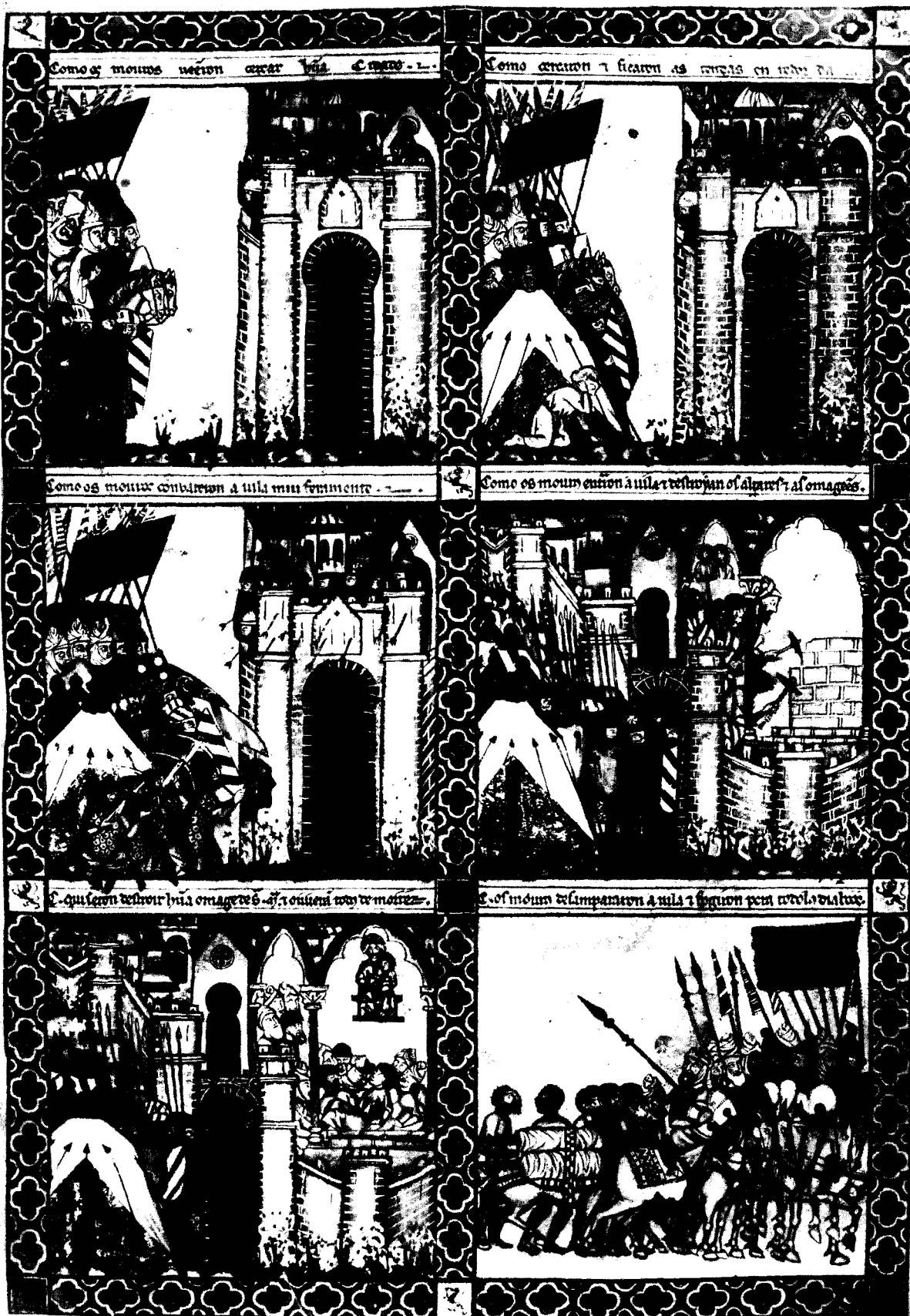
Cântiga 46. Mass conversion by Virgin's flow of milk.



Cântiga 63. Virgin helps warrior who misses battle.



Cántiga 83. Christian captive in southern Spain.



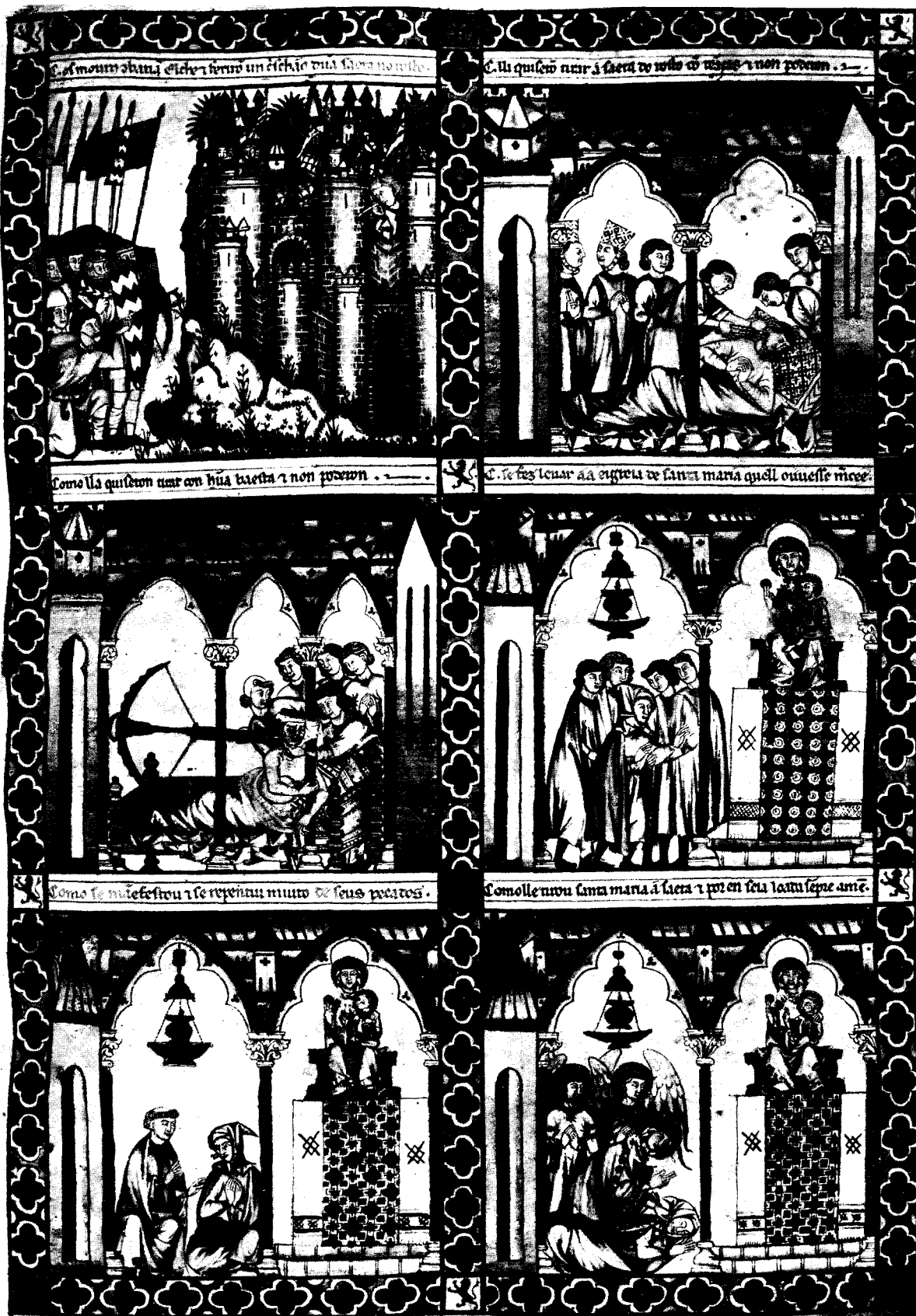
Cântiga 99. Triumphant invaders flee Virgin's vengeance.



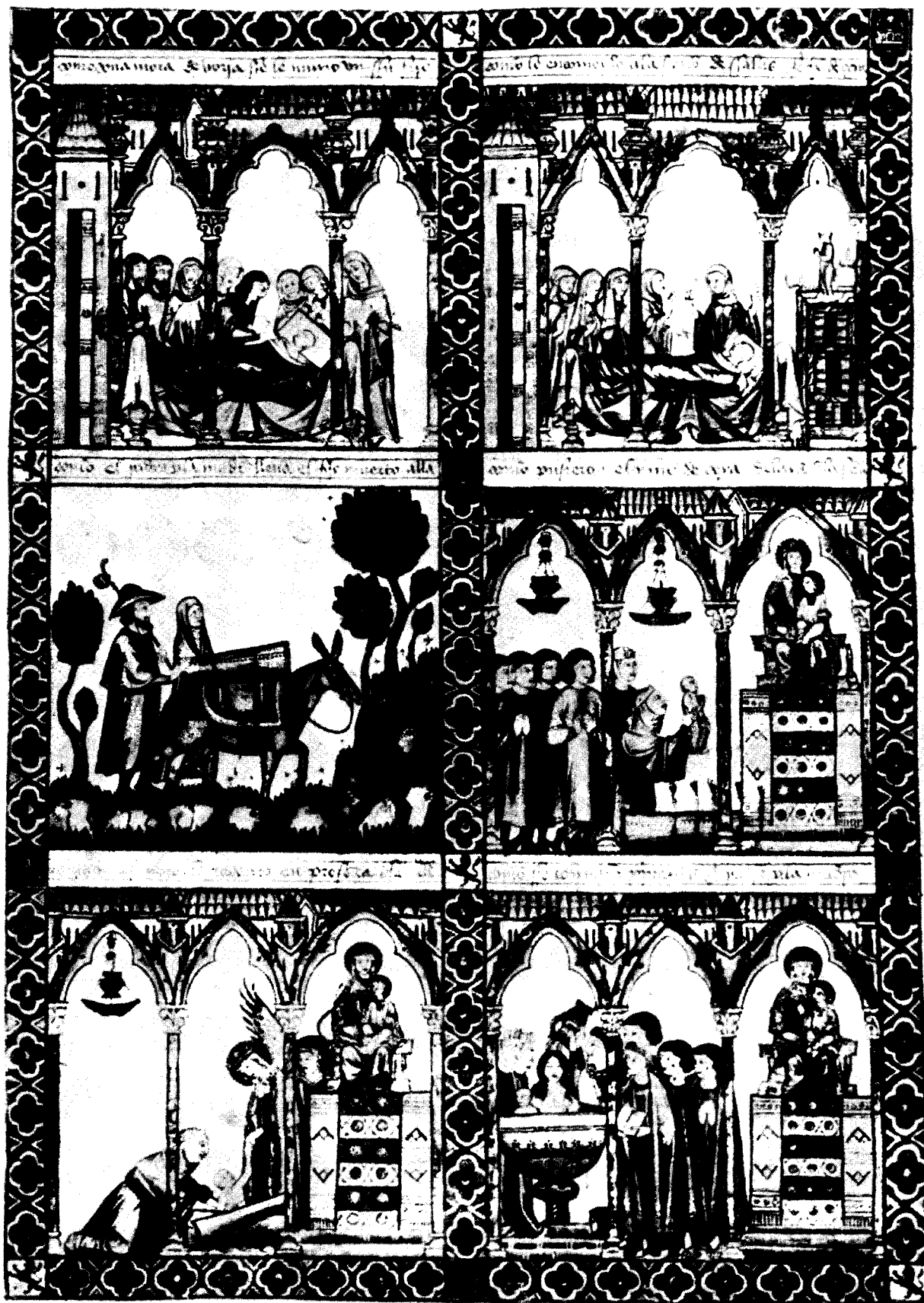
Cântiga 95. Hermit prevents Islamic fleet from ravaging his region. (pt. 1



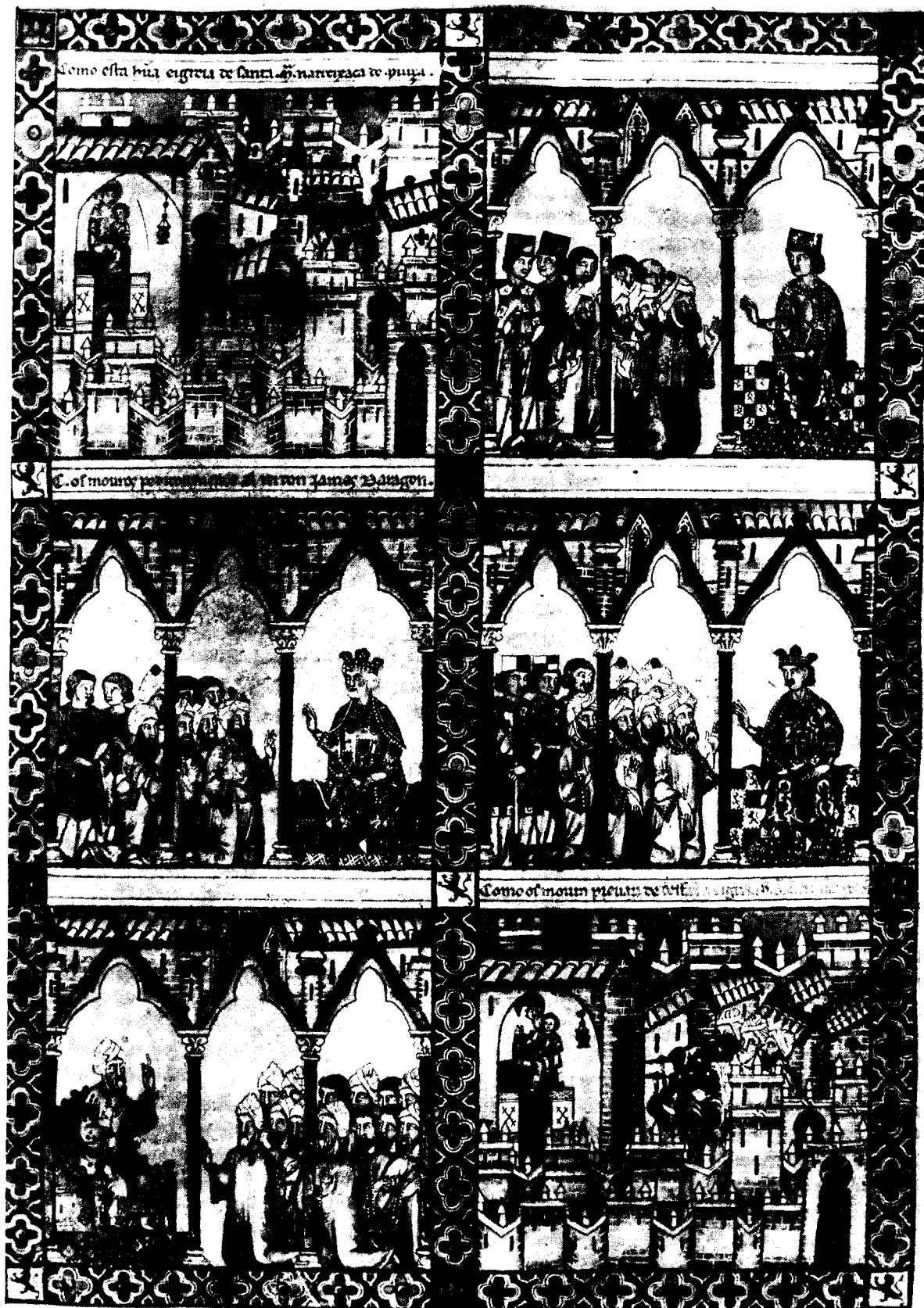
Cântiga 95. (pt. 2.)



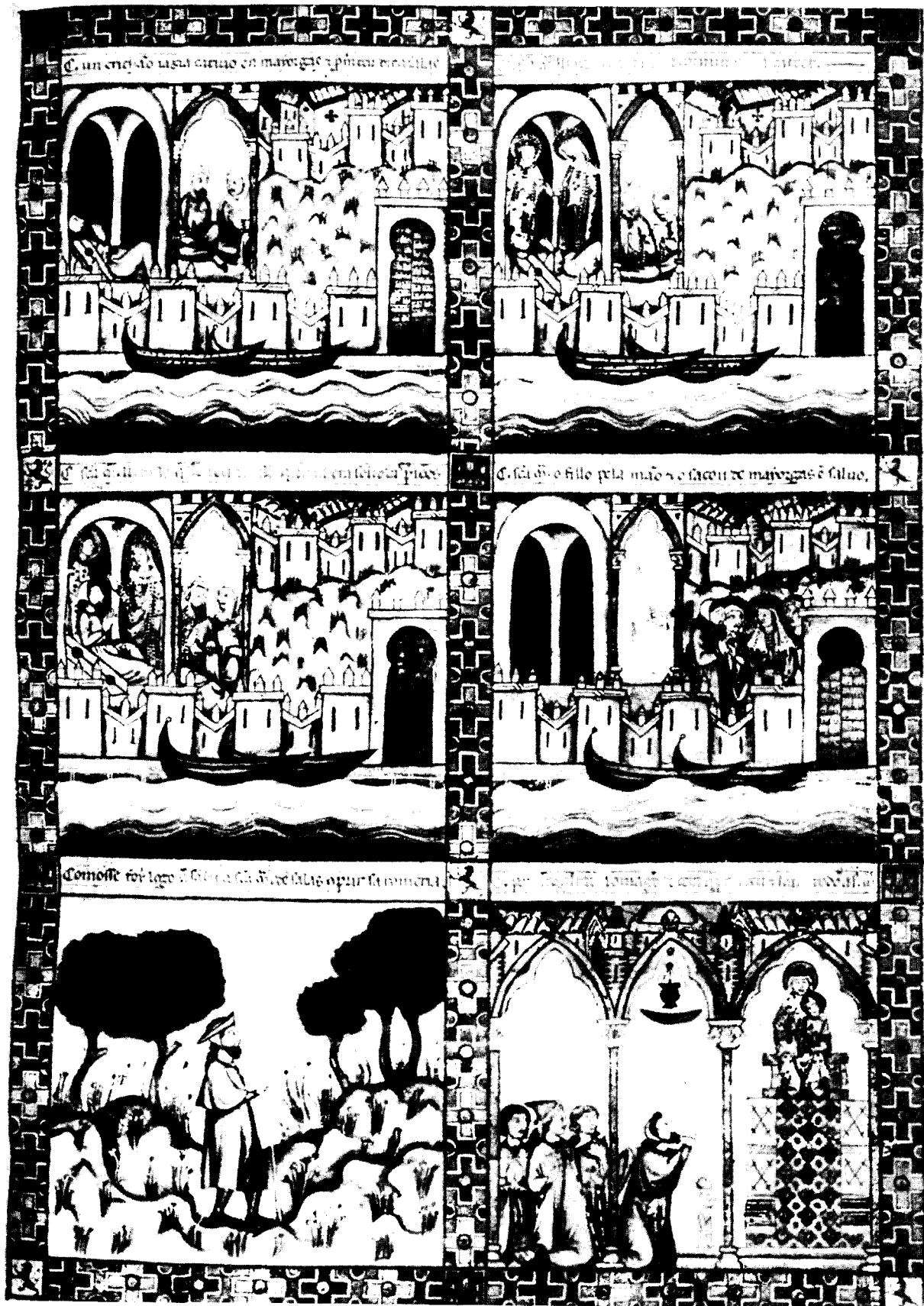
Cántiga 126. Moorish crossbowman wounds Elche citizen.



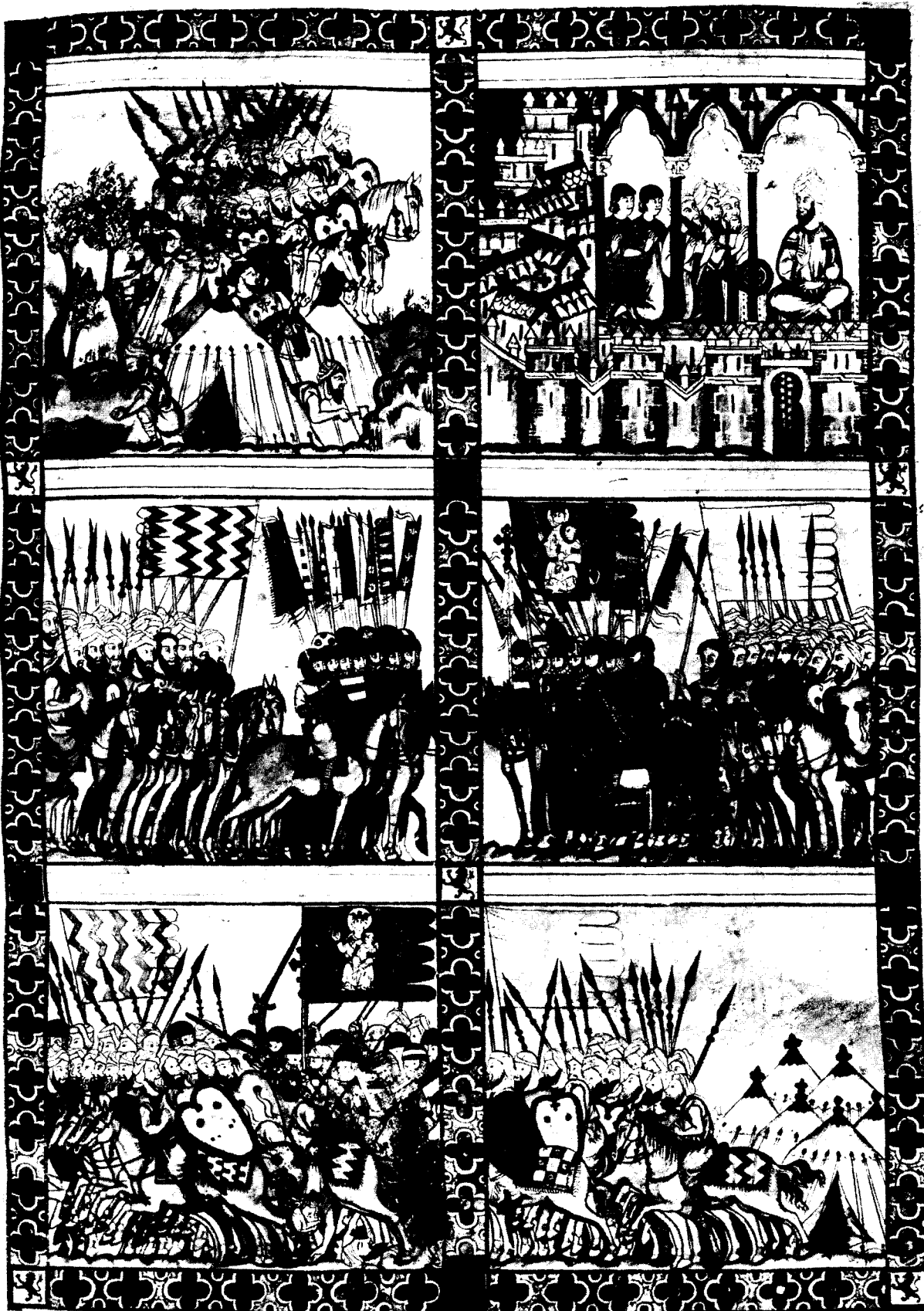
Cántiga 167. Conversion of Aragonese Mudejar family by resurrection.



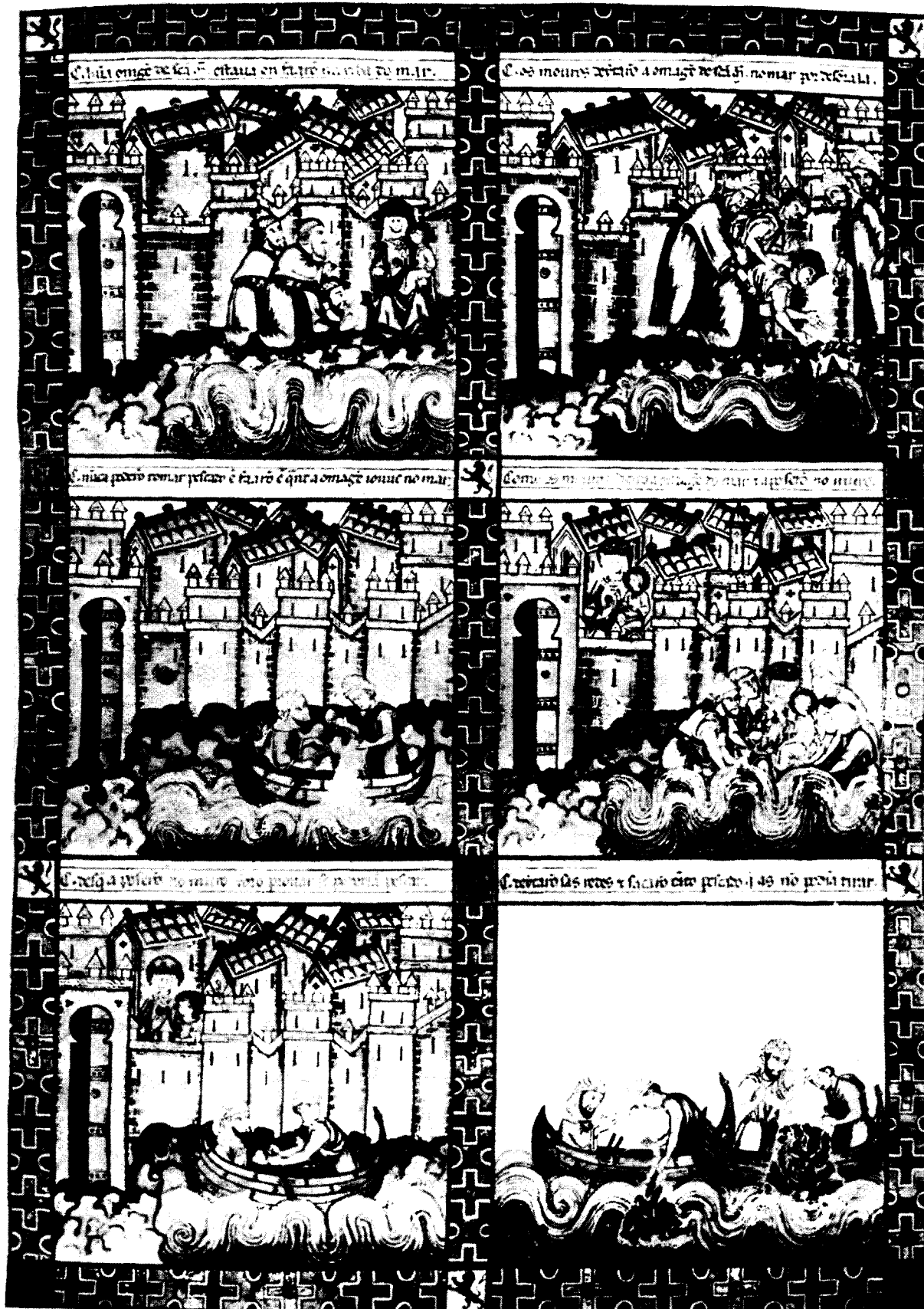
Cántiga 169. Murcian Moors cannot destroy hated church.



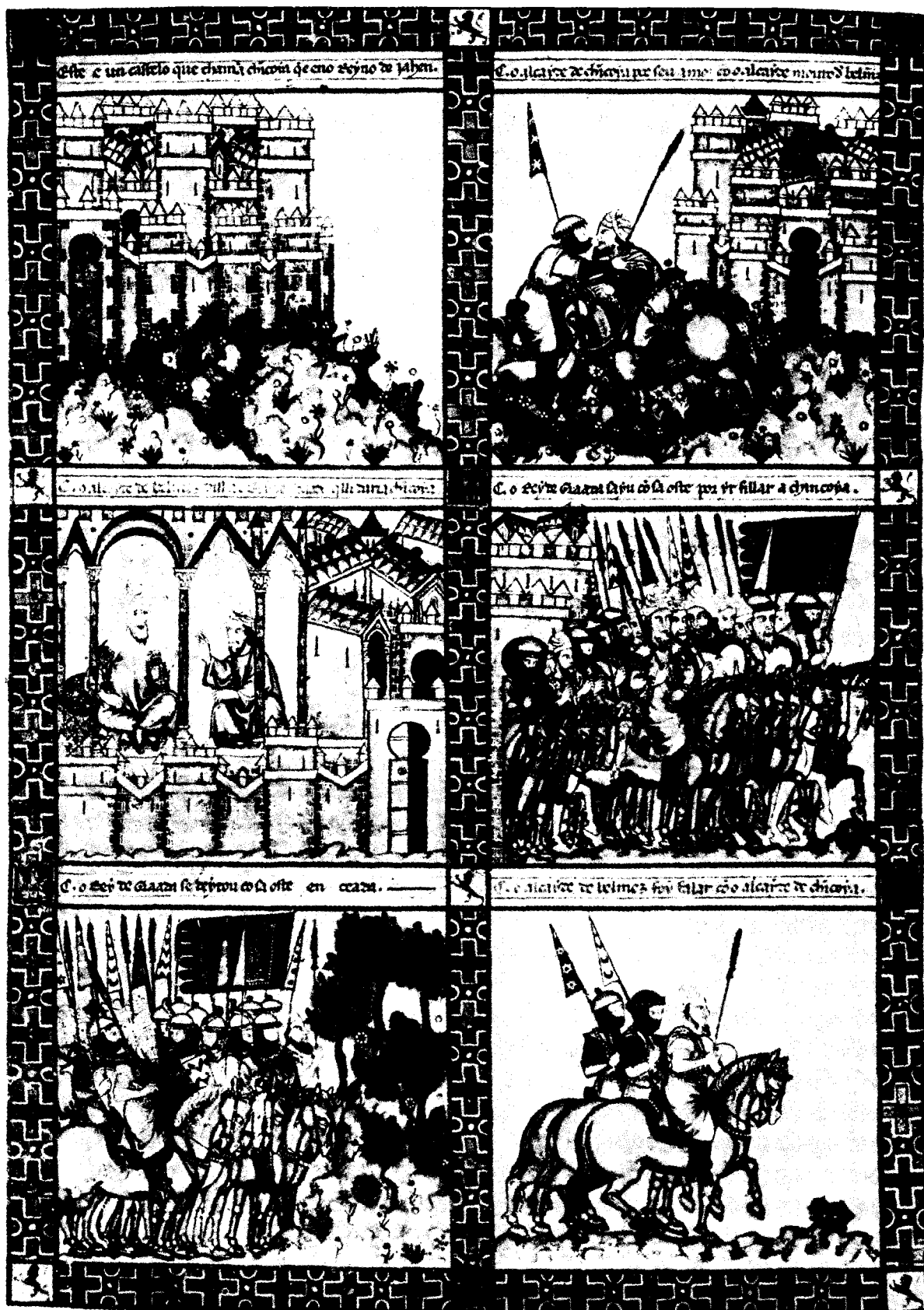
Cântiga 176. Captive escapes Islamic Majorca.



Cántiga 181. Muslims battle Muslims in Morocco.



Cântiga 183. Muslim fishermen suffer disaster.



Cântiga 185. Granada king driven from a Jaén castle. (pt. 1.)



Cântiga 185. (pt. 2.)



Cántiga 192. Conversion of a Negro Muslim from Almeria.

Cántiga 46. Mass conversion by Virgin's flow of milk.

Muslim raiders overrun Christians, seizing cattle (A), returning joyfully (B) to divide the loot, including arms, vessels, and cloth (C). The leader chooses a painting of the Virgin, visiting it daily in his home with reverence (D). The image feeds the child; milk flows from both breasts (E), thus converting the entire region (F). Note four Negroes (A, C), turbans for dignitaries (C), flocks of goats, sheep, and cows (B), the medieval nude and the baptismal form (F), the European armament favored by many Spanish Moors (A), and the Arabic inscription on the left-hand curtain (D). Themes include mass conversion, climate of violence, the "good" Muslim.

Cántiga 63. Virgin helps warrior who misses battle.

The local lord, Count García, welcomes a pious but absent-minded knight (with zebra-striped contingent), come to help defeat al-Manṣūr (A). Missing the battle because of his custom of hearing three Masses (B), he is saved from embarrassment by a miraculous counterpart who leads the action (C, D), and is hailed as hero by the returning count (E), so all thank the Virgin (F). Note how the action "crosses" to make one picture of C and D, and the fine Negro head (D). Themes include war as piety, chivalric shame, and the Moor-Christian struggle.

Cántiga 83. Christian captive in southern Spain.

Raiders snatch a Christian from Lucena into captivity (A), mistreating him (B). His prayers to the Virgin (C) strike the irons from his neck and hypnotize the guards, and he walks out carrying his chains (D) as an ex-voto to leave at Mary's shrine, where he dictates an account to a scribe (E), offering a Mass of thanksgiving as church bells ring (F). Note costume detail and two Moors without the usual beards (A), the "double action" where the protagonist appears twice, and the Negroes as relief guards (D). Themes include mistreatment to speed ransom, church bells hated by Muslims and correspondingly cherished by Christians, and captivity as daily peril for both peoples.

Cántiga 99. Triumphant invaders flee Virgin's vengeance.

Muslims siege a Spanish city (A), raising their tents and encircling the defenders (B), attacking fiercely (C), until they break in, destroying altars and images (D). While attempting to harm the Virgin's image some conquerors drop dead (E); the rest flee the town fearfully (F). Note the Negro heads, one with white beard (D, F), infantry forces and banners (A), shield positions and the crossbow contingents for which Spanish Islam was noted (C), the Moorish gate on the Christian town and other architectural details (A, D), pickaxes (D), portable tents (F), and Arabic inscription on the banner (C, F). Themes include iconoclasm, enemy aggression, and divine protection against Islam.

NOTE: The panel sequence within each episode is identified alphabetically (A through F). For *Cántigas* 95 and 185, each of which is shown in two parts, the sequence of panels in part 2 is represented by the letters G through L.

Cántiga 95. Hermit prevents Islamic fleet from ravaging his region.

A rare double set of twelve panels shows a German count settling at a seaside shrine in Portugal (A) to serve the Virgin as a holy hermit (B), generously feeding her pilgrims (C) until captured while fishing (D). With the hermit below decks (E), the raiding fleet is constantly blown back to the same spot (F). The Muslims bring him up (G); offer gold and gifts; he accepts a crystal goblet (H); they restore him to the shrine, never afterwards daring to molest him (I). They go home (J); the hermit spreads the story (K); and crowds increase at the shrine (L). Note eating utensils (C), Negro portraits (D, F, J), details of galley construction, and the theme of sea peril.

Cántiga 126. Moorish crossbowman wounds Elche citizen.

At Elche in Murcia, at the frontier of the Valencia kingdom, a Muslim crossbowman pierces a citizen's neck (A). Physicians prove unable to remove the quarrel or arrow-bullet by forceps (B), nor can they shoot it out with a crossbow reversed (C). Carried to a Marian shrine (D) the victim repents and prays (E) until the Virgin appears with two angels to cure him (F). Note the banner with Arabic and the characteristic palms designating Elche. Themes include the east coast wars, the feared crossbow contingents, and the perils of frontier life.

Cántiga 167. Conversion of Aragonese Mudejar family by resurrection.

A Mudejar family at Borgia in Aragon sees its infant son die (A), but has recourse to the Virgin, fashioning a wax image of their child as ex-voto (B). Father and mother transport the coffin on muleback (C) to put the corpse and ex-voto at the Virgin's shrine, where they spend the night (D). On the third day the Virgin revives the child (E), which results in the family's conversion (F). Note the women's dress and hair (A, F), the Europeanized Mudejar styles in the north (C), and the manner of baptizing (F). Themes include conversion and pilgrimage.

Cántiga 169. Murcian Moors cannot destroy hated church.

A Marian church in the Arrijaca quarter of Islamic Murcia attracts Genoese, Pisan, and Sicilian merchants (A). Prince Alfonso of Castile masters Murcia and protects the church (B). King James of Aragon, after subduing a revolt (for Castile), and as recompense for making the major mosque the cathedral, yields to the Muslims' plea to have this church destroyed (C). Unable, they later win this privilege from Alfonso, now Castile's king (D), but their puppet-king Ibn Hūd refuses confirmation, reverencing Mary (E). During a revolt they labor to harm the church and again cannot (F). Note the contemporary portraits and costume of James, Alfonso, and Ibn Hūd (B, C, D, E) and the puppet's throne or Hispano-Islamic chair and curled shoes (E). Themes include Murcia's conquest and reconquest and enclave churches under Islam and Christendom.

Cántiga 176. Captive escapes Islamic Majorca.

A Christian languishing in prison on this Balearic island promises a pilgrimage if freed (A). The Virgin appears (B) and leads him safely through the Islamic city

and away (C, D). Bearing a wax image of himself as ex-voto, the pilgrim fulfills his vow (E, F). Note light boats (*batel* type as in *Cántiga* 183) (A to D), unveiled women (A to C), types of headdress (C), and city battlements. Themes include Islamic Balearics and captivity.

Cántiga 181. Muslims battle Muslims in Morocco.

Opposing forces preparing for battle in Morocco are encamped (A) under their respective Muslim leaders (B). One faction fights under the Virgin's banner (C, D) and, of course, wins (E, F). Note the setting up of tents (A), the heraldry and armament (E, F), the ruler's pillow unlike the Murcian Muslim's throne in *Cántiga* 169 (B), and the two examples of crossover action shared by neighboring panels (C–D, E–F). Themes include nearby North African politics, internecine Islamic strife, and preconversion dispositions of “good” Moors.

Cántiga 183. Muslim fishermen suffer disaster.

A stone statue of the Virgin at the seashore near the Islamic town of Faro (A) is contemptuously thrown into the sea by Muslims (B). The town's fishermen can make no catch (C), until the culprits recover the statue and display it honorably on the town wall (D). The fish return immediately (E), in greater abundance than ever (F). Note fishing boats, nets, costume, and technique (C, E, F) and architectural details. Themes include iconoclasm, coastal fishing, and anti-Christian feeling.

Cántiga 185. Granada king driven from a Jaén castle.

This double set of panels is sited at Chincoya castle in Jaén (A), whose castellan is a friend of the Moorish castellan of Belmez (B). The Belmez lord can therefore inform Granada's king that only fifteen men garrison Chincoya, low on rations (C). Granada's king sallies to seize this prize (D, E). Meanwhile the Belmez Moor rides out with two of his knights (F) to meet his Christian friend (G), but kidnaps him (H). Granadan armies siege Chincoya (I), but the Virgin's image brought to the battlements routs them (J), defending the castle like a good castellan (K), until the Granadan king ignominiously flees (L). The song's refrain has an oddly modern ring: “Power to St. Mary!” Note the king's pillow-throne, the Arabic on Granada's banner, and the heraldic flags. Themes include Granadan frontier warfare, Christian-Muslim friendship, and treachery.

Cántiga 192. Conversion of a Negro Muslim from Almeria.

A good Christian lord unflinchingly attempts to convert his unbelieving captive from Almeria (A). In prison the Muslim wrestles with and bites the devil (B). An apparition of the Virgin rescues the Moor, urging conversion (C). The grateful Negro tells the lord, who adds his own pleas (D), until the captive seeks baptism (E), becoming a fervent Christian (F). Note the Negro portrait, especially in D, and compare the baptismal scene with previous baptisms. Themes include the captive Muslims and zeal for conversion.

[Text continued from p. 1412]

themselves taking the only ground on which Muslim leaders would wholeheartedly meet. The conviction speaks volumes about the Mudejar intellectual scene.

Ironically it was a pious Muslim, not the busy Dominicans, who left us our only glimpse into the system actually at work. Ibn Rashīq al-Mursī, a beardless adolescent assisting his father in drafting notarial documents at Murcia city around 1250, fell into controversy with a priest at the local center. He later recalled how a routine oath in connection with a lawsuit between a Muslim and a Christian brought him to the ample residence (*dār*) and church of the infidel missionaries. They comprised

a group of priests and monks dedicated, according to them, to the devout life and to studying the sciences, but interested above all in the sciences [learning] of the Muslims and in translating them into their language with the object of criticizing them—the most high God frustrate their projects!—eager to engage in polemic with Muslims, intending to lure the weak to their side.

This description hardly fits the short-lived *madrassa* patronized by Alfonso the Learned, whose chief ornament was the good Muslim Muhammad b. Ahmad b. Abī Bakr al-Riqūṭī al-Mursī and a school where Christians and Jews enrolled along with the Muslim student body; nor did diocesan authorities in this era have at their disposition such a body of learned “priests and monks.”

Ibn Rashīq encountered there “a priest from Marrakesh, eloquent, widely knowledgeable, and moderate in discussion,” who not only knew Arabic perfectly but was versed in the Koran, Arabic literature, and scholasticism. The Muslim found him patient, tolerant, and free from fanaticism. This may have been the great Martí, or less probably the diplomat and archdeacon of Morocco García Pérez, or merely one of the local Dominican staff. The priest praised the boy, remarking that he had heard of the father’s erudition and of the youth’s promise as a bright student. Invited to enter amiable debate, Ibn Rashīq plunged into an extended discussion of miracles and related themes. The experience impressed the young Muslim profoundly. Years later, perhaps after he had become *kātib* to the emir of Ceuta or after his subsequent return to Granada, he composed a record of the long dialogue, preserved centuries afterward in a chance copy by another scholar. Beyond the bleak administrative records, this episode reveals something of the manner of approach and inner strength gracing the institution.¹⁰⁴

The duration of the Valencian schools, continuing into the opening years of

¹⁰⁴ Al-Ḥusayn b. ʿAtīq b. al-Ḥusayn b. Rashīq al-Taglibī al-Mursī, tr. and introd. by Fernando de la Granja as “Una polémica religiosa en Murcia en tiempos de Alfonso el Sabio,” *Al-Andalus*, 31 (1961): 47–72, the text, from al-Wansharīsī (d. 1508), comprising pp. 67–72. The time was after the first or tributary conquest (1243) and before the death of the father (1263). Ibn Rashīq was still living in 1275–76; if his beardless adolescence situates the episode around 1250, my attribution of an early date for Murcia’s school gains strength; in any case, this disposes of Coll’s date of 1266.

the fourteenth century, betrays the presence of a larger class of erudites than historians have supposed. The future of the schools apparently fell under dispute before the end of the first decade; after 1313 they mysteriously disappear from the provincial records. Training continued as before, Coll argues, but dispersed now in the Dominican philosophy faculties of the Crown of Aragon. The reorganization, carried through only a short time after the upgrading of the *Játiva arabicum* in 1312, marks a coming of age for the schools and a passing of the crash-program phase. It may also mark a diminution or near-end of the Mudejar erudite class in the Valencian kingdom. It probably represents a reorientation away from special concern for the Spanish frontier toward fresher fields. The academic reshuffling, however, did not terminate the importance of Játiva and Valencia city as local centers of conversion.¹⁰⁵

There is room for a more pessimistic view. Charles Dufourcq contends that the dream of converting North Africa dimmed in the first quarter of the fourteenth century and that now more Christians were being lost than Muslims gained; an era of harsher military crusading and commercial problems was at hand.¹⁰⁶ If his interpretation is just, Christian attitudes in the Mudejar regions and Dominican attitudes in general must have reflected the change. Some commentators, assessing the whole enterprise of medieval intellectual missions, complain that the polemicists never really entered the Islamic mind as sympathetic ecumenists. The schoolmen would have thought the reproach irrelevant, since their specialized mission assumed that the Muslim intellectual at bottom could hardly take the dogmas of Islam seriously.

It is likely that the work of these centers influenced the complimentary view Ibn Khaldūn took of Christian philosophical activity. He first drafted his major work in Tunisia from 1375 to 1378; though he wrote a lifetime after the *studia* disappeared his focus was that of a Tunisian with chauvinistic pride in his Spanish origins and a sharp awareness of the Valencian crusade era. As diplomat to King Peter of Castile in 1363–64 he was offered his family's Seville property, which had been confiscated at their flight over a century before. Tunis, he reminds us, had served as the central receiving area for Valencian and Murcian Mudejar emigrants. It had also housed the main Dominican schools that later retreated to the Valencia-Murcia territory. Ibn Khaldūn understood "that the philosophical sciences are greatly cultivated in the land of Rome and along the adjacent shore of the country of the European Christians." The "adjacent shore" included, but must have stressed, the realms of Aragon. Ibn Khaldūn used the term "Rome" in traditional fashion for Byzantium, but more often as the capital city and symbol for European Christendom. His shorthand use of it here for Europe is peculiarly

¹⁰⁵ On the transformation see also Coll, "Escuelas de lenguas orientales," 18 (1945): 81, 85–87; 19 (1946): 238–39. The *acta* of the provincial chapter of 1313, perhaps critical for our knowledge here, have disappeared.

¹⁰⁶ Dufourcq, *L'Espagne catalane*, 580–82, 585–86.

just, since the philosophic apostolate among erudites was commissioned and encouraged by the popes, who showed lively interest in the conversion of Tunis. The philosophical sciences "are said to be studied there again and to be taught in numerous classes," Ibn Khaldūn continues; "existing systematic expositions of them are said to be comprehensive, the people who know them numerous, and the students of them very many." This paragraph reflects the brief but real rupture in the mutual insulation of Muslim and Christian intellectual societies, a bridging of learned languages and alien mentalities. Ibn Khaldūn exhausted his interest in Christian metaphysical activity in those sentences, breaking off with the pious tag: "God knows better what exists there."¹⁰⁷

AS TIME PASSED the dream of conversion flickered, fitfully dimmed, and died. It could not survive the harsh realities, for it rested on unfirm foundations—that Muslims stood at the brink of conversion, that their savants could scarcely credit Islamic beliefs, and that key princes inclined toward Christianity. It rested, too, upon an aggressive optimism—sometimes upon an eschatological euphoria, including influences from Joachimite ideology and from the Mongol irruption—that faded as the factors comprising the medieval balance shifted toward the turn of the century, giving way to problems, doubts, rebuffs, and general decline of spirit that the crisis of the Black Death at the following mid-century merely culminated and crowned. The dream rested, too, upon a situation within Islam, shattered and shaken and on the defensive, which inevitably readjusted, reacted, and grew again in strength. Lull again came reluctantly to admit the need for armed crusade, making of war a second foot for holding up the crippled dream. For a moment of time, nonetheless, influential people had favored sheathing the sword, sitting down in dialogue with the immemorial and hated enemy; for a moment, many men had groped for some common ground that was not a battlefield. The dream failed. It had amounted to a reaffirmation of a traditional, more profoundly Christian approach to the dissident. By the end of the century, though Christendom had put on an armor of inquisition and was entering an era of punitive harshness, the memory of the century-long effort remained quiescent in mendicant tradition. New Worlds, a century or two later, revived the dream in fresh forms; for the moment anyway it had entered the Western spirit as a corrective tropism. And if it contributed nothing more lasting than to encourage Aquinas in elaboration of his open-minded *Contra gentiles*, mankind is the richer.

¹⁰⁷ Ibn Khaldūn, *The Muqaddimah: An Introduction to History*, tr. Franz Rosenthal (New York, 1958), 3: 117–18. Ibn Khaldūn was not being sarcastic; he commonly ends a discussion with some appropriate tag.

Populist Dreams and Negro Rights: East Texas as a Case Study

LAWRENCE C. GOODWYN

NEARLY A CENTURY LATER the Populist decade lingers in historical memory as an increasingly dim abstraction. The very word "Populism" no longer carries specific political meaning. It is now invoked to explain George Wallace, as it was used to explain Lyndon Johnson in the sixties, Joe McCarthy in the fifties, and Claude Pepper in the forties. Though afflicting principally the popular mind, this confusion is at least partly traceable to those historians who have insisted on concentrating on Populism as exhortation, so that Ignatius Donnelly's utopian novels or Mary Lease's pronouncements on the respective uses of corn and hell become the explanatory keys to agrarian radicalism. For scholars who mine political movements with a view to extracting cultural nuggets, the focus has been chiefly upon the word, not the deed; in the process the agrarian crusade has become increasingly obscure.¹

Much of the difficulty centers on the subject of race. There is essential agreement that, on economic issues, Populists were men of the Left, primitive to some, prophetic to others, but leftists to all. But did their banner indicate a highly selective nativist radicalism for whites only, or did they grapple with the inherited legacies of the caste system as part of an effort to

¹ Such careful inquiries as C. Vann Woodward, *Origins of the New South* (Baton Rouge, 1951); Woodward, *Thomas Watson, Agrarian Rebel* (New York, 1938); and Walter T. K. Nugent, *The Tolerant Populists* (Chicago, 1963), demonstrate how regional and state studies can reconstruct the milieu within which men performed their public political labors. Both historians are careful to set the words of Populists, Democrats, and Republicans against their respective acts. In contrast Richard Hofstadter and Norman Pollack, though in healthy disagreement in their assessment of the quality of Populist agitation, both rest their analysis on elusive cultural and ideological categories that often seem far removed from the inner workings of the agrarian crusade. In *The Populist Response to Industrial America* (Cambridge, 1962), Pollack strains to find an authentic socialist basis for Populist criticisms of American capitalism. The attempt has the effect of diminishing the provincial generosity and innocence of Populism as well as socialist claims to ideological consistency; it also carries Pollack's inquiry toward the upper reaches of the party hierarchy in a manner frequently unrelated to the substance of third-party survival at the local level. The scholarly assault on Hofstadter's *The Age of Reform* (New York, 1958) has been both telling and recurring—the recurrence a testament to the vitality of this creative and persuasively written book. The criticism that Hofstadter selected a small number of Populist writings as a basis for sweeping generalizations about the nature of the agrarian crusade remains as true as ever.

create what they considered a more rational social and economic order? The analysis of Populist rhetoric has left us with contradictory answers.

While party platforms can be useful tools in determining professed attitudes, the gap between asserted ideals and performance is sufficiently large to defeat any analysis resting on the implicit assumption that political manifestos have an intrinsic value apart from the milieu in which they existed. In America the distance between assertion and performance is especially evident in matters of race; as a result, on this issue above all, the context of public assertions is central to the task of their political evaluation.² An inquiry into the murkiest corner of Populism, interracial politics, should begin not merely with what Populists said but what they did in the course of bidding for power at the local level. What was the stuff of daily life under Populist rule in the rural enclaves where the third party came to exercise all the authority of public office, including police authority? What can we learn not only about Populist insurgency but also about the orthodoxy the third party opposed?

GRIMES COUNTY, TEXAS, was one of many counties scattered across the South and West where the People's party achieved a continuing political presence in the latter part of the nineteenth century. Located some sixty miles north of Houston in the heart of what the natives call the Old South part of Texas, Grimes County displayed the cotton-centered economy typical of rural East Texas in 1880. Its largest town, Navasota, contained 1,800 persons in 1890 and its second largest town, Anderson, the county seat, only 574 persons as late as 1900. Farms in Grimes County ranged from plantation size in the rich bottomland country of the Brazos River on the county's western border to small, single-family agricultural units on the poorer land of the northern part of the county.³ The 1890 census revealed a county population of 21,312, of which 11,664 were black.⁴

Populism in Grimes County is the story of a black-white coalition that had its genesis in Reconstruction and endured for more than a generation.

² For example, a central aspect of race relations in the South concerns the question of which classes in Southern society took the lead in the successive processes—black disfranchisement being one of the more essential ones—by which the antebellum caste system, in altered form, was reinstitutionalized after Reconstruction. Analysis of rhetoric that is not intimately related to these processes as they occur cannot be expected to produce evidence that bears on the crucial causal relationships involved. In this connection a recent study by William I. Hair touches directly on one of these processes—the violent suppression of black trade unionism. Hair asserts that the gentry “embraced the kind of Negrophobia elsewhere usually attributed to ignorant poor whites.” When Louisiana planters crushed a Knights of Labor strike in the lower delta parishes in 1887 casualties among cane field workers “ran into the hundreds.” *Bourbonism and Agrarian Protest: Louisiana Politics, 1877–1900* (Baton Rouge, 1969), 184.

³ The author wishes to acknowledge the assistance of Marcus Mallard of Navasota, chairman of the Grimes County Historical Society. Mr. Mallard provided social, economic, and genealogical information on the county and many of its prominent families.

⁴ Bureau of the Census, *Thirteenth Census of the United States, Abstract with Supplement for Texas* (Washington, 1913), 620; *Texas Almanac, 1910* (Dallas, 1910), 133.

In time this coalition came to be symbolized by its most enduring elected public official, Garrett Scott. The Scotts had roots in Grimes County dating back before the Civil War. Their sons fought for the Confederacy and returned to face a postwar reality by no means unique in the South; possessing moderately large holdings of land but lacking necessary capital to make it productive, the Scotts did not achieve great affluence. During the hard times that continued to afflict undercapitalized Southern agriculture through the 1870s Garrett Scott became a soft-money agrarian radical.⁵ His stance was significant in the political climate of Grimes County in the early 1880s. During Reconstruction Negroes in the county had achieved a remarkably stable local Republican organization, headed by a number of resourceful black leaders. When Reconstruction ended and white Democrats regained control of the state governmental machinery in Texas, Grimes County blacks retained local power and sent a succession of black legislators to Austin for the next decade.⁶ The local effort to end this Republican rule took the usual postwar Southern form of a political movement of white solidarity under the label of the Democratic party. In supporting the Greenback party Garrett Scott not only was disassociating himself from the politics of white racial solidarity, he was undermining it.

In 1882 a mass meeting of various non-Democratic elements in Grimes County nominated a variegated slate for county offices. Among the candidates were black Republicans, "lily-white" Republicans, and Independent Greenbackers. Garrett Scott was on the ticket as the Independent Greenback candidate for sheriff.⁷ Not much is known about the racial climate in Grimes County in 1882, but it must not have been wholly serene, because the "lily-white" nominee for county judge, Lock MacDaniel, withdrew from the ticket rather than publicly associate with black candidates.⁸ Garrett Scott did not withdraw, and in November he was elected. Also elected, as district clerk, was a black man who became a lifelong political ally of Scott, Jim Kennard.⁹ Thus began an interracial coalition that endured through the years of propagandizing in Texas by the increasingly radical Farmers Alliance and through the ensuing period of the People's party. The success of the coalition varied with the degree of white participation. After the collapse of the Greenback party in the mid-eighties visible white opposition to the Democratic party declined for several years before Grimes County farmers, organized by the Alliance, broke with the Democracy to form the nucleus of the local People's party in 1892. Scott and Kennard were the most visible symbols of the revitalized coalition, but there were others as well. Among them were Morris Carrington, a Negro school principal, and Jack Haynes,

⁵ *Galveston News*, Sept. 10, 1882; *Navasota Tablet*, Nov. 11, 1900.

⁶ Harrell Budd, "The Negro in Politics in Texas, 1877-1898" (master's thesis, University of Texas, 1925), 83; J. Mason Brewer, *Negro Legislators of Texas* (Dallas, 1935), 64, 74-75, 81.

⁷ *Galveston News*, Sept. 10, 1882.

⁸ *Ibid.*, Sept. 21, 1882.

⁹ *Ibid.*, Nov. 11, 1882.

both staunch advocates of Populism in the black community, as well as J. W. H. Davis and J. H. Teague, white Populist leaders. These men led the People's party to victory in the county elections of 1896 and again in 1898.¹⁰

A subtle duality creeps into the narrative of events at this point. To the world outside Grimes County in the 1890s, to both Populists and Democrats, Garrett Scott was simply another Populist officeholder, distinguished for his antimonopoly views and his generally radical approach to monetary policy. To his white supporters within Grimes County he was doubtless respected for the same reasons. But to the Democrats of Grimes County the sheriff symbolized all that was un-Southern and unpatriotic about the third party. Under Populist rule, it was charged, Negro school teachers were paid too much money; furthermore, in Scott's hands the sheriff's office hired Negro deputies. The two Democratic newspapers in Navasota were fond of equating Populist rule with Negro rule and of attributing both evils to Scott. The Navasota *Daily Examiner* asserted that "the Negro has been looking too much to political agitation and legislative enactment. . . . So long as he looks to political agitation for relief, so long will he be simply the means of other men's ambition."¹¹ To the Navasota *Tablet* Scott was simply "the originator of all the political trouble in Grimes County for years."¹² Both these explanations oversimplify Grimes County politics. The political presence and goals of blacks were definite elements of local Populism, as was, presumably, the personal ambition of Garrett Scott. But the Populists' proposed economic remedies had gained a significant following among the county's white farmers, and this was of crucial importance in inducing white Populists to break with Democrats and ally themselves with blacks. Garrett Scott was a living embodiment of white radicalism; he did not cause it.¹³ Beyond this the political cohesion of blacks was a local phenomenon that

¹⁰ Carrington and Haynes as well as Kennard had been active in the county Republican organization prior to the emergence of the third party. The information from contemporary sources on the political lives of Negro leaders in Grimes County that was used in this paper was augmented by oral interviews with their descendants. The author wishes to express his gratitude to Maurice Lyons and B. T. Bonner, both former students at the University of Texas, for their assistance in the conduct of oral interviews in the black communities of Navasota, Anderson, Plantersville, and Richards in Grimes County. Largely through the efforts of Mr. Lyons and Mr. Bonner, the author was able to locate the descendants of every known black leader of the People's party in Grimes County. With respect to the third party's white leadership, the political histories of Teague, Davis, and Scott, traced through both oral interviews and contemporary sources, stand as examples of the diverse sources of Southern Populism. Teague, like Scott, spent his entire political life in opposition to the Democratic party—but as a Republican rather than as an agrarian radical. Quietly progressive on the race issue, Teague possessed considerable administrative talents and eventually became chairman of the third party for the first congressional district of Texas. He was elected county judge in 1896 and was re-elected in the local third-party sweep of 1898. Davis, a Democrat, became quite radical on economic issues, broke with his party, and became a third-party editor. He displayed an ambivalent stance on the race issue and was not prominent in the events described in this paper.

¹¹ Navasota *Daily Examiner*, Oct. 13, 1898.

¹² *Tablet*, Nov. 11, 1890.

¹³ The characterization of third-party rule as "Negro rule" was common in the Democratic press in counties where Populism was strong. Such accounts must be weighed against other stories, appearing in the same newspapers, that acknowledged the strong appeal of the People's party among white farmers. In this connection, see the *Examiner*, Nov. 4, 1898.

had preceded Scott's entry into Grimes County politics and had remained relatively stable since the end of the war. The ease with which Democratic partisans saw the fine hand of Garrett Scott in Negro voting was more a reflection of their own racial presumptions than an accurate description of the political dynamics at work in the county.

Through the election of 1898 Democrats in Grimes County had labored in vain to cope with the disease of Populism among the county's white farmers. Finally, in the spring of 1899, the Democrats moved in a new direction. The defeated Democratic candidate for county judge, J. G. McDonald, organized a clandestine meeting with other prominent local citizens and defeated Democratic office seekers. At this meeting a new and—for the time being—covert political institution was created: the White Man's Union. A charter was drawn providing machinery through which the Union could nominate candidates for county offices in elections in which only White Man's Union members could vote. No person could be nominated who was not a member; no person could be a member who did not subscribe to these exclusionary bylaws; in effect, to participate in the organization's activities, so adequately expressed in its formal title, one had to support, as a policy matter, black disfranchisement.¹⁴ Throughout the summer and fall of 1899 the White Man's Union quietly organized.

Writing years later McDonald explained that care was taken not to launch the organization publicly "until the public attitude could be sounded."¹⁵ By January 1900 the covert organizing had been deemed sufficiently successful to permit the public unveiling of the White Man's Union through a long story in the *Examiner*. During the spring the *Examiner's* political reporting began to reflect a significant change of tone. In April, for example, the *Examiner's* report of a "quiet election" in nearby Bryan noted that friends of the two mayoral candidates "made a display of force and permitted no Negroes to vote. All white citizens went to the polls, quietly deposited their ballots for whom they pleased and went on about their business."¹⁶ The *Examiner* had progressed from vague suggestions for disfranchisement to approval of its forcible imposition without cover of law.

The first public meetings of the White Man's Union, duly announced in the local press,¹⁷ occupied the spring months of 1900 and were soon augmented by some not-quite-so-public night riding. The chronology of these events may be traced through the denials in the local Democratic press of their occurrence. In July the *Examiner* angrily defended the county's honor against charges by the Negro Baptist State Sunday School Conference that the county had become unsafe for Negroes. The *Austin Herald* reported from the state's capital that the Sunday School Board, "after mature thought and

¹⁴ The bylaws of the White Man's Union were published in the *Examiner*, Jan. 6, 1900.

¹⁵ J. G. McDonald to E. L. Blair, July 10, 1928, in E. L. Blair, *Early History of Grimes County* (Austin, 1930), 197.

¹⁶ *Examiner*, Apr. 4, 1900.

¹⁷ *Ibid.*, Apr. 2, 3, June 4, 6, 11, July 17, 18, 19, 20, 30, 1900.

philosophical deliberation," had decided to cancel its annual meeting scheduled for Navasota.¹⁸ The *Examiner* cited as "irresponsible slush" the charge that Negroes were being threatened and told to leave the county, but within weeks reports of just such events began cropping up in the *Examiner* itself.¹⁹ One example of terrorism left no one in doubt, for it occurred in broad daylight on the main street of the county seat: in July Jim Kennard was shot and killed within one hundred yards of the courthouse. His assailant was alleged to be J. G. McDonald.²⁰

Intimidation and murder constituted an even more decisive assault on the People's party than had the ominous bylaws of the White Man's Union. The Populist leadership recognized this clearly enough, and Scott went so far as to attempt to persuade Southern white farmers to shoulder arms in defense of the right of Negroes to vote.²¹ Beyond this we know little of the measures attempted by the local Populist constabulary to contain the spreading terrorism. A well-informed member of the Scott family wrote a detailed account of these turbulent months, but the manuscript was subsequently destroyed. In the early autumn of 1900 members of the White Man's Union felt sufficiently strong to initiate visits to white farmers with a known allegiance to the People's party. Under such duress some of these farmers joined the White Man's Union.²²

In August the Union, aided by a not inconsiderable amount of free publicity in the local press, announced "the Grandest Barbecue of the Year," at which the "workings of the White Man's Union" would be explained to all. The leadership of the People's party objected to announced plans to include the local state guard unit, the Shaw Rifles, in the program. After some discussion the Texas adjutant general, Thomas Scurry, placed at the discretion of the local commander the question of the attendance of the Shaw Rifles

¹⁸ Austin *Herald*, reprinted in *Examiner*, July 17, 1900.

¹⁹ *Examiner*, Sept. 4, 13, Oct. 19, Nov. 5, 1900.

²⁰ Carrie Meacham, private interview near Plantersville, Texas, Aug. 12, 1970. Mrs. Meacham is the daughter of the slain Populist leader. W. F. McGowan, private interview in Navasota, Apr. 14, 1970. Mr. McGowan, now ninety-four years old, was a personal friend of Jim Kennard. A. P. Wickey, private interview in Anderson, May 14, 1970. Mr. Wickey is the source of the statement attributing Kennard's death to Judge McDonald. Mr. Wickey's stepfather was a prominent member of the White Man's Union; the younger Wickey, now in his eighties, was present in Anderson the day of the slaying. His account is supported by Mrs. Meacham: "Judge McDonald shot my father off his horse on the main street of Anderson."

²¹ The Navasota *Tablet* accused Scott of attempting to rally Populists in defense of Negro voting rights, describing his public appeals as "raving speeches." *Tablet*, Nov. 11, 1900.

²² Edith Hamilton, private interview in Richards, Texas, May 24, 1970. Though specific information about the night-riding activities of the White Man's Union can occasionally be found in the local Democratic press, that source cannot be characterized as zealous in its reporting of extraparlimentary aspects of the campaign of 1900. Accounts of intimidation of Negro Populists have been preserved in the oral tradition of Grimes County Negroes; accounts of intimidation of white Populists have been preserved in the oral tradition of the Scott family. Mrs. Hamilton, now eighty years of age, is the niece of Garrett Scott. Richards, Texas, is located in Grimes County, a few miles from the county seat of Anderson. The lost "Populist history" of Grimes County was written by Mrs. Hamilton's father. It was destroyed after his death by his wife, Cornelia Kelly, because, says Mrs. Hamilton, "my mother felt we had all suffered enough and no purpose would be served by keeping my father's manuscript."

in a body. The commander, Captain Hammond Norwood, a leading Navasota Democrat and a member of the White Man's Union, exercised his option, and the Shaw Rifles appeared en masse at the function. Populist objections were brushed aside.²³

Shortly after this well-attended barbecue had revealed the growing prestige of the White Man's Union as well as the inability of the People's party to cope with the changing power relationships within the county, a black exodus began. People left by train, by horse and cart, by day and by night. The *Examiner*, with obvious respect for the new political climate its own columns had helped engender, suggested elliptically that the exodus could produce complications. Some citizens, said the *Examiner*, "are beginning to feel a little nervous as the thing progresses, and lean to the idea that the action will bring on detrimental complications in the labor market."²⁴

The next day, however, the paper printed a public address that it said had been "ordered published by the executive committee of the White Man's Union in order to combat the many reports that are calculated to injure the Union." After reaffirming the Union's intent to end "Negro rule" in the county, the report concluded with a message "to the Negroes":

Being the weaker race, it is our desire to protect you from the schemes of those men who are now seeking to place you before them. . . . Therefore, the White Man's Union kindly and earnestly requests you to keep hands off in the coming struggle. Do not let impudent men influence you in that pathway which certainly leads to trouble. . . . In the future, permit us to show you, and convince you by our action, that we are truly your best friends.²⁵

Fourteen days later a black Populist leader, Jack Haynes, was riddled with a shotgun blast by unknown assailants. He died instantly in the fields of his cotton farm.²⁶

²³ *Examiner*, July 30, Aug. 8, 17, 18, 24, 1900. The affair of the Shaw Rifles was described in the *Examiner*, Aug. 21, 1900. The *Examiner* had by this stage become quite committed to the cause of extraparliamentary disfranchisement. On August 24 the paper described the White Man's Union picnic in terms of triumph, asserting that five thousand people had feasted at "1500 feet of tables . . . laden with well-turned and thoroughly seasoned barbecue, pork and mutton." Replying a week later to out-of-town dispatches that Grimes County politics had become complicated by the presence of four political tickets (Democratic, Republican, Populist, and White Man's Union), the paper replied: "Grimes County is in better shape politically than most counties in Texas. There is only one ticket and one piece of a ticket in the field. Anyone who viewed the Anderson picnic parade last week would have left little room for doubt as to which side would win." *Examiner*, Aug. 31, 1900.

²⁴ *Ibid.*, Sept. 13, 1900.

²⁵ *Ibid.*, Sept. 14, 1900. The promptness of the reply by the White Man's Union to the *Examiner's* gentle admonition may be taken as an indication of the confidence and aggressiveness of the organization's leadership.

²⁶ Jack Haynes, Jr., private interview, Navasota, Texas, Apr. 14, 1970. Mr. Haynes is the son of the slain Populist leader. W. F. McGowan, interview, Apr. 14, 1970. The *Examiner*, Sept. 27, 1900, carried a one-paragraph story on Haynes's murder without, however, attributing to it any political implications. Haynes was not identified as a Populist leader. The murder of another black Populist leader, Morris Carrington, was also reported in the same issue, again without specifying Carrington's role in the People's party. This report had no foundation in

The White Man's Union held a rally in Navasota two nights later that featured a reading of original poetry by one of the Union's candidates, L. M. Bragg. The verse concluded:

Twas nature's laws that drew the lines
Between the Anglo-Saxon and African races,
And we, the Anglo-Saxons of Grand Old Grimes,
Must force the African to keep his place.²⁷

Another White Man's Union rally held in Plantersville the same week displayed other Union candidates whose conduct won the *Examiner's* editorial approval: "They are a solid looking body of men and mean business straight from the shoulder."²⁸ Apparently this characterization of the Plantersville speakers was not restricted to approving Democrats; Populists, too, responded to events initiated by the men who "meant business." In October the Plantersville school superintendent reported that only five white families remained in his school district and that all the Negroes were gone. The superintendent stated that twelve white families had left that week, and "the end is not in sight."²⁹

Amid this wave of mounting terror the People's party attempted to go about its business, announcing its nominating conventions in the local press and moving forward with the business of naming election judges and poll watchers. But there were already signs of a fatal crack in Populist morale. The People's party nominee for county commissioner suddenly withdrew from the race. His withdrawal was announced in the *Examiner*, and no explanation was offered.³⁰

Throughout the late summer and autumn of 1900 the demonstrated power of the White Man's Union had protected McDonald from prosecution in the Kennard slaying. Nothing short of a war between the Populist police authority and the White Man's Union could break that extralegal shield. An exasperated and perhaps desperate Garrett Scott angrily challenged a White Man's Union official in October to "go and get your Union force, every damn one of them, put them behind rock fences and trees and I'll fight the whole damn set of cowards."³¹ That Scott had to use the first person singular to describe the visible opposition to the Union underscores the extent to which terror had triumphed over the institutions of law in Grimes County. By election eve it was clear that the Populist ticket faced certain defeat. The third party had failed to protect its constituency. White

fact and was printed either through error or by design to frighten the county's black population. Mr. Carrington died in 1923. The value of received oral traditions in correcting primary—and partisan—sources is briefly discussed at the conclusion of this paper.

²⁷ *Examiner*, Sept. 29, 1900.

²⁸ *Ibid.*

²⁹ *Ibid.*, Oct. 24, 1900.

³⁰ *Ibid.*, Oct. 30, 1900.

³¹ *Tablet*, Nov. 11, 1900.

Populists as well as black were intimidated. Many would not vote; indeed, many were no longer in the county.³²

Over 4,500 votes had been cast in Grimes in 1898. On November 6, 1900, only 1,800 persons ventured to the polls. The People's party received exactly 366 votes. The Populist vote in Plantersville fell from 256 in 1898 to 5 in 1900. In the racially mixed, lower-income precinct of south Navasota the Populist vote declined from 636 to 23. The sole exception to this pattern came in a geographically isolated, lower-income precinct in the extreme northern part of the county that contained few Negroes and thus, presumably, fewer acts of terrorism. The Populist vote in this precinct actually increased from 108 to 122 and accounted for one-third of the countywide vote of 366. In north Navasota, also almost all white but not geographically isolated from the terror, the Populist vote declined from 120 to 3.³³ An additional element, nonstatistical in nature, stamped the election as unusual. The underlying philosophy of the South's dominant political institution, the Democratic party, has perhaps never been expressed more nakedly than it was in Grimes County in 1900 when "the party of white supremacy," as C. Vann Woodward has called the Southern Democracy, appeared on the official ballot as the White Man's Union.³⁴

On the way to its landslide victory the Union had grown more self-confident in its willingness to carry out acts of intimidation and terrorism in defiance of the local Populist police authority. Now that that authority had been deposed and a sheriff friendly to the White Man's Union had been elected, would terrorism become even more public?

On November 7, 1900, the morning after the election, a strange tableau unfolded on the streets of Anderson, the tiny county seat.³⁵ Horsemen began arriving in town from every section of the county, tied their horses all along the main street, and occupied the second floor of the courthouse. In a

³² The *Examiner's* pre-election issue foresaw a "quiet election" despite "some unmistakable bitterness in some quarters." The paper reported that "everything points to the success of the White Man's Union ticket." *Examiner*, Nov. 5, 1900.

³³ *Examiner*, Nov. 10, 1898, Nov. 9, 1900. Official Texas election returns are available in the state archives only on a countywide basis.

³⁴ The twenty-five per cent decline in the Democratic vote showed that not everyone was wholly content with the climate of violence that had developed. The *Examiner* somewhat opaquely expressed this anxiety. After noting that the Negro exodus was not confined to Grimes County, the White Man's Union and its tactics having spread to other counties, the newspaper felt constrained to add: "Yet there is a positive indication that something deep is at the bottom of the removal—some source for the frightful, unchristian and willful fabrications circulated." *Examiner*, Nov. 5, 1900. The *Examiner* can perhaps be pardoned for its failure to comment on its own role as a "source" if not of fabrications then of the advantages of the exclusionary administration of the ballot.

³⁵ The ensuing account of the events of November 7-11 is derived from a variety of sources. Both Navasota newspapers published versions of the Anderson affair, the *Tablet*, in a lengthy story on November 11 and the *Examiner* on November 8-10. The *Galveston News* carried increasingly detailed accounts on November 8-12. In addition to those persons cited elsewhere herein, a number of Grimes County residents supplied information on a basis not for attribution. In the black community the effect of the terrorism of 1900 has not yet run its course. The adjutant general's account, which is available in the Texas State Archives, Austin, is quite brief. *Report of the Adjutant General, 1899-1900* (Austin, 1900).

nearby house Garrett Scott's sister, Cornelia, and her husband, John Kelly, watched the buildup of Union supporters on the courthouse square, not fifty yards from the sheriff's official residence on the second floor of the county jail. They decided the situation was too dangerous to permit an adult Populist to venture forth, so the Kellys sent their nine-year-old son with a note to warn Scott not to appear on the street.

At about the same time that this mission was carried out Garrett Scott's younger brother, Emmett Scott, came into town from the family farm, rode past the growing clusters of armed men, and reined up in front of the store belonging to John Bradley, his closest friend in town. Bradley was a Populist but, as befitting a man of trade, a quiet one. His store was adjacent to the courthouse.

Cornelia Kelly's son found the sheriff at Abercrombie's store across the street from the jail and delivered the warning note. As Scott read it an outbreak of gunfire sounded from the direction of Bradley's store. Scott stepped to the street and peered in the direction of the fusillade. Rifle fire from the second floor of the courthouse immediately cut him down. Upon hearing the gunfire Cornelia Kelly ran out of her house and down the long street toward the courthouse. The gunsights of scores of men tracked her progress. Seeing her brother's body in the street she turned and confronted his attackers. "Why don't you shoot me, too," she yelled, "I'm a Scott." She ran to her brother and, with the assistance of her son, dragged him across the street to the county jail. He was, she found, not dead, though he did have an ugly wound in his hip. Inside Bradley's store, however, three men were dead—Emmett Scott, Bradley, and Will McDonald, the son of a Presbyterian minister and a prominent member of the White Man's Union. McDonald had shot Scott shortly after the latter had entered the store; the two men grappled for the gun, and the fatally wounded Scott fired one shot, killing McDonald. Bradley was killed either by a shot fired from outside the store where Union forces had gathered near the courthouse or by a stray bullet during the struggle inside.³⁶

The siege of Anderson continued for five days, with the wounded sheriff and his deputies—black and white—in the jail and the White Man's Union forces in the courthouse. Shots crossed the fifty yards between the two buildings intermittently over the next several days. On the evening of the fatal shooting another member of the Scott clan, Mrs. W. T. Neblett, had left

³⁶ The *Tablet* leaves open the question of how Bradley's death occurred. White oral tradition holds that Scott killed Bradley. This is disputed by the Scott family oral tradition, supplied by Mrs. Hamilton, that Bradley was Scott's "best friend." The *Galveston News* supports Mrs. Hamilton's version: "As a result of some words, McDonald emptied his revolver into Emmett Scott, killing him, hitting him every time. He grabbed Scott's pistol, and the two began scuffling when a shot rang out and Bradley fell." *News*, Nov. 8, 1900. A subsequent bulletin, also printed in that issue, revises the story: "It was first thought Bradley received an accidental shot from Scott's pistol but later reports say he was shot by someone else. It is claimed Bradley had nothing to do with the fight between Scott and McDonald." The *News* described all three victims as men "prominent in the county."



The scene of the postelection gun battle between Populists and supporters of the White Man's Union. The photograph was taken in about 1915 and is an accurate picture of Anderson at the time of the battle. The courthouse is at the end of Main Street. Photograph courtesy of Barker History Center, University of Texas.

Navasota for Austin to plead with the governor, Joseph D. Sayers, for troops. On Friday she returned, accompanied by the adjutant general of the State of Texas, Thomas Scurry—the same official who had earlier acquiesced in the participation of the state guard in the White Man's Union barbecue. After conferring with the contending forces Scurry pondered various methods to get the wounded Scott out of town and into a hospital; gangrene had set in. For protection, Scurry suggested that he be authorized to select a group of twenty prominent citizens of Navasota to escort the sheriff from the jail to the railroad station. Since most of the "prominent citizens" of Navasota were members of the White Man's Union, it is perhaps understandable that Scott declined this offer. The adjutant general then suggested that the Shaw Rifles be employed as an escort. This idea was respectfully declined for the same reason. Asked what he would consider a trustworthy escort, the wounded sheriff suggested a state guard unit from outside the county.³⁷

On Saturday, four days after the shooting, a company of Houston light infantry of the Texas Volunteer State Guard detrained at Navasota and marched the eleven miles to Anderson. On Sunday morning Garrett Scott was placed on a mattress, the mattress put in a wagon, and the proces-

³⁷ *Report of the Adjutant General*, 12. Both the *Tablet*, November 11, 1900, and Mrs. Hamilton agree in principle on this summation of the conversation between Garrett Scott and the adjutant general.

sion began. In the wagon train were most of the members of the large Scott clan—Emmett Scott's widow and children, the Kelly family, and the Nebletts, all with their household belongings piled in wagons. A file of infantrymen marched on either side as the procession formed in front of the jail, moved past hundreds of armed men at the courthouse and onto the highway to Navasota, and then boarded a special train bound for Houston.³⁸

THUS DID POPULISM leave Grimes County. From that day in 1900 until well after mid-century Negroes were not a factor in Grimes County politics. J. G. McDonald regained his judgeship and served for many years. The White Man's Union continued into the 1950s as the dominant political institution in the county. None of its nominees, selected in advance of the Democratic primary, was ever defeated.³⁹ The census of 1910 revealed the extent of the Negro exodus. It showed that Grimes County's Negro population had declined by almost thirty per cent from the 1900 total.⁴⁰ School census figures for 1901 suggest an even greater exodus.⁴¹

To this day the White Man's Union, as a memory if no longer as an institution, enjoys an uncontested reputation among Grimes County whites as a civic enterprise for governmental reform. In this white oral tradition the general events of 1900 are vividly recounted. Specific events are, however remembered selectively. The exodus of Negroes from the county is not part of this oral tradition, nor is the night riding of the White Man's Union or the assassination of the Negro Populist leaders.

As for Garrett Scott, he endured a long convalescence in a San Antonio hospital, regained his health, married his nurse, and moved to a farm near Houston. He retired from politics and died in his bed. He is remembered in the oral tradition of the black community as the "best sheriff the county ever had." Kennard and Haynes were killed because they "vouched" for Scott among Negroes.⁴² In this black oral tradition the Negro exodus plays a central role. It is perhaps an accurate measure of the distance between

³⁸ *Galveston News*, Nov. 12, 1900; *Tablet*, Nov. 11, 1900; *Report of the Adjutant General* says that eight men and six women had taken refuge in the jail (p. 12).

³⁹ On this point all oral traditions in Grimes County correspond.

⁴⁰ *Thirteenth Census*, 822. The Negro population declined from 14,327 in 1900 to 9,858 in 1910. In 1890 the black population of Grimes had been 11,664.

⁴¹ *Scholastic Population and Apportionment of Available School Fund for 1901* (Austin, 1901), 7. While school census figures are available for 1901, I have been unable to locate comparable data for 1900. Nearest available figures prior to 1901 are for 1889. The 1901 school census, though taken a year after the exodus and presumably reflecting the return of some Negroes in addition to in-migration encouraged by the labor shortage, reveals a decline in the number of Negro pupils of fifteen per cent from the 1889 total, despite the fact that census returns show an increase of almost twenty per cent in Negro population between 1890 and 1900. This comparison suggests that the thirty per cent decline in Negro population evident from the census returns for 1900 and 1910 probably substantially minimizes the actual exodus that occurred in the late summer and fall of 1900. An exodus in the range of from forty to fifty per cent probably would be a reasonable estimate.

⁴² W. F. McGowan, interview, Apr. 14, 1970.

the races in Grimes County today that two such contradictory versions of famous events could exist side by side without cross-influence.

To these two oral traditions a third must be added—the Scott tradition. The Scotts were, and are, a proud family. One by one, as they died, they were brought home to be buried in the family plot in the Anderson cemetery, little more than a mile from the site of the bloody events of 1900. Tombstones of female members of the clan bear the Scott middle name, defiantly emblazoned in marble. Edith Hamilton of Richards, Grimes County, was ten years old in November 1900 and remembers vividly the day her nine-year-old brother carried her mother's message to Garrett Scott. She remembers the defiance of her mother, the political commitment of her father, the acts of intimidation by the White Man's Union, the Negro exodus, and what she calls the "intelligence of Uncle Garrett." "They said that Uncle Garrett was a nigger-lover," recalls Mrs. Hamilton. "He wasn't a nigger-lover, or a white-lover, he just believed in being fair to all, in justice."⁴³

The Scott oral tradition—similar to the black oral tradition and at odds with the white tradition—is virtually the only legacy of the long years of interracial cooperation in Grimes County. Beyond this the substance of political life that came to an end in Grimes County in 1900 cannot be measured precisely from the available evidence. Very little survives to provide insight into the nature of the personal relationship that existed between Garrett Scott and Jim Kennard, between any of the other Populist leaders of both races, or between their respective constituencies. Scott and his third-party colleagues may have been motivated solely by personal ambition, as the White Man's Union charged; on the other hand, the impulses that made them Populists in the first place may have led them toward public coalition with blacks. It is clear that such stridently white supremacist voices as the *Navasota Tablet* were unable to project any reason other than personal ambition to explain the phenomenon of white men willingly associating themselves politically with black men. To what extent this attitude reflected Populist presumptions is another question. White Populists and black Republicans shared an animosity toward the Southern Democracy that grew in intensity during the bitter election campaigns of the 1890s. Democratic persistence in raising the cry of "Negro domination" to lure Populist-leaning voters back to the "party of the fathers" was effective enough to keep white Populists on the defensive about the race issue throughout the agrarian revolt in the South. The circumstance of a common political foe nevertheless provided Populists and Republicans with a basis for political coalition that was consummated in a bewildering variety of ways—and sometimes not consummated at all. The stability of local black organizations and their demonstrated capacity to withstand Democratic blandish-

⁴³ Edith Hamilton, interview, May 13, 1970.

ments or acts of intimidation were only two of the factors governing the complex equation of post-Reconstruction interracial politics. A stable, local black political institution existed in Grimes County, and its enduring qualities obviously simplified the organizational task confronting Garrett Scott. What might be regarded as "normal" Bourbon efforts to split blacks from the Populist coalition—mild intimidation, petty bribery, campaign assertions that the Democrats were the Negroes' "best friends," or a combination of all three—failed to achieve the desired results in Grimes County in the 1890s. The precise reasons are not easily specified. The *Navasota Tablet*, seeing the world through lenses tinted with its own racial presumptions, ascribed the credit for Negro political cohesion solely to the white sheriff. In the face of all Democratic stratagems, the third party's continuing appeal to Negroes was, in the *Tablet's* view, a thing of "magic." A white supremacist view does not automatically exclude its holder from rendering correct political analyses on occasion, and it is possible that the *Tablet's* assessment of the cause of Negro political solidarity was correct; however, such an analysis does not explain how the Negro Republican organization was able to send a succession of black legislators to Austin in the 1870s and 1880s, before Garrett Scott became politically active. It seems relevant that when Grimes County Democrats decided upon an overt campaign of terrorism, the men they went after first were the leading black spokesmen of Populism in the county rather than the third party's white leadership. To this extent the actions of Democratic leaders contradicted their public analysis of the causal relationships inherent in the continuing Populist majorities.

Before they indulged in terrorism the Democrats already possessed another method of splitting the Populist coalition: regaining the loyalty of white Populists. Against the historic Democratic campaign cry of white supremacy, the People's party had as its most effective defense the economic appeal of its own platform. The persuasiveness of Populism to white farmers in Grimes County was confirmed by newspaper accounts of the public reaction to the Populist-Democratic debates that occurred during the years of the agrarian uprising. While the reports in the *Examiner* were uniformly partisan and invariably concluded that Democratic spokesmen "won" such debates hands down, the papers conceded that Populist speakers also drew enthusiastic responses from white residents. The absence of reliable racial data by precincts renders a statistical analysis of the Populist vote in Grimes County impossible; however, the fragmentary available evidence suggests that the People's party was generally able to hold a minimum of approximately thirty per cent of the county's white voters in the four elections from 1892 to 1898 while at the same time polling approximately eighty to ninety per cent of the Negro electorate. The inability of the Democratic party to "bloc vote" the county's white citizenry, coupled with the party's failure to win black voters by various means or, alternatively, to diminish the size of the Negro electorate, combined to ensure Democratic defeat at the polls. The

fact merits emphasis: both the cohesion of black support for the People's party and the maintenance of substantial white support were essential to the local ascendancy of Populism.

This largely deductive analysis, however, reveals little about the internal environment within the third-party coalition during the bitter struggle for power that characterized the decade of Populist-Democratic rivalry. However scrutinized, the bare bones of voting totals do not flesh out the human relationships through which black and white men came together politically in this rural Southern county. In the absence of such crucial evidence, it seems prudent to measure the meaning of 1900 in the most conservative possible terms. Even by this standard, however, a simple recitation of those elements of Grimes County politics that are beyond dispute isolates significant and lasting ramifications.

An indigenous black political structure persisted in Grimes County for thirty-five years following the Civil War. Out of his own needs as a political insurgent against the dominant Southern Democratic party, Garrett Scott decided in 1882 to identify his Greenback cause with the existing local Republican constituency. Once in office as sheriff he found, among other possible motives, that it was in his own self-interest to preserve the coalition that elected him. It is clear that the style of law enforcement in Grimes County under Scott became a persuasive ingredient in the preservation of black support for the People's party. The presence of black deputy sheriffs and Scott's reputation within the black community seem adequate confirmation of both the existence of this style and its practical effect. The salaries paid Negro school teachers constituted another element of third-party appeal. Comparisons with white salaries are not available, but whatever black teachers received, partisans of the White Man's Union publicly denounced it as "too much." It is evident that Grimes County Negroes supported the People's party for reasons that were grounded in legitimate self-interest—an incontestable basis for political conduct. The point is not so much that the county's Negroes had certain needs, but that they possessed the political means to address at least a part of those needs.

From this perspective the decisive political event of 1900 in Grimes County was not the overwhelming defeat of the local People's party but the political elimination of that part of its constituency that was black. Scott was valuable to Negroes in short-run terms because he helped to translate a minority black vote into a majority coalition that possessed the administrative authority to improve the way black people lived in Grimes County. In the long run, however, it was the presence of this black constituency—not the conduct of a single white sheriff nor even the professed principles of his political party—that provided the Negroes of the county with what protection they had from a resurgent caste system. As long as Negroes retained the right to cast ballots in proportion to their numbers they possessed bargaining power that became particularly meaningful on all occasions when whites

divided their votes over economic issues. Disfranchisement destroyed the bargaining power essential to this elementary level of protection. Arrayed against these overriding imperatives for Negroes such questions as the sincerity of Garrett Scott's motives fade in importance. Whatever the sheriff's motives, both the political realities that undergirded the majority coalition and Scott's ability to respond to those realities shaped a course of government conduct under the People's party that was demonstrably of more benefit to Negroes than was the conduct of other administrations before or since. The permanent alteration of those realities through black disfranchisement ensured that no other white administration, whether radical, moderate, or opportunistic, would be able to achieve the patterns in education and law enforcement that had come to exist in the county under Populism. Stated as starkly as possible, after 1900 it was no longer in the interest of white politicians to provide minimal guarantees for people who could not help elect them.

Beyond this crucial significance for the county's black people, disfranchisement also institutionalized a fundamental change in the political environment of whites. More than a third party passed from Grimes County in 1900; in real political terms an idea died. Though a new political idea invariably materializes in democratic societies as an expression of the self-interest of a portion of the electorate, the party that adopts the idea in the course of appealing for the votes of that sector of the electorate inevitably is placed in the position of having to rationalize, defend, explain, and eventually promote the idea. If the concept has substance, this process eventually results in the insinuation of the idea into the culture itself. In this sense it is not necessary to know the precise depth of the commitment to Negro rights of the Grimes County People's party to know that the *idea* of Negro rights had a potential constituency among white people in the county as long as black people were able to project its presence through their votes. Given the endurance of this real and potential constituency, one could reasonably intuit that twentieth-century politics in Grimes County would have contained one, or a dozen, or a thousand Garrett Scotts—each more, or less, "sincere" or "ambitious" than the Populist sheriff. Disfranchisement destroyed the political base of this probability. A political party can survive electoral defeat, even continuing defeat, and remain a conveyor of ideas from one generation to the next. But it cannot survive the destruction of its constituency, for the party itself then dies, taking with it the possibility of transmitting its political concepts to those as yet unborn. It is therefore no longer possible to speak of two white political traditions in Grimes County, for the White Man's Union succeeded in establishing a most effective philosophical suzerainty. Seventy years after disfranchisement Mrs. Hamilton can recall the racial unorthodoxy of Uncle Garrett; she cannot participate in such activity herself. "The Negro people here don't want this school integration any more than the whites do," she now says. "They're not ready

for it. They don't feel comfortable in the school with white children. I've talked to my maid. I know."⁴⁴

While Garrett Scott's memory has been preserved, the local presence of the creed of his political party died with the destruction of that party. There has been literally no political place to go for subsequent generations of Scotts and Teagues, or Kennards and Carringtons. This absence of an alternative political institution to the Democratic party, the party of white supremacy, has been a continuing and unique factor in Southern politics.⁴⁵ The circumstance is based on the race issue, but in its long-term political and social implications it actually transcends that issue.

THE POPULIST ERA raises a number of questions about the interaction of the two races in the South, both within the third party and in the larger society. It is widely believed, by no means merely by laymen, that after the failure of Reconstruction meaningful experiments with the social order were finished in the South and that the aspirations of blacks were decisively thwarted. The example of Grimes County suggests, however, the existence of a period of time—a decade perhaps, or a generation—when nascent forms of indigenous interracial activity struggled for life in at least parts of the old Confederacy. Was some opportunity missed and, if so, how? How widespread through the South, and the nation, was this opportunity?

The White Man's Union was organized and led by men who considered themselves the "best people" of the South. If this attitude was typical, major adjustments must be made in our understanding of precisely how, and for what reasons, the antebellum caste system, in altered form, was reinstitutionalized in Southern society a generation after the formal ending of slavery. Was the "red-neck" the source of atrocity, or was he swept along by other stronger currents? And what of the Populist role? To what extent was agrarian racial liberalism in Texas traceable to an overall philosophy within the third-party leadership? Through what intuition of self-interest did the radical organizers of the Farmers Alliance, the parent institution of the People's party, accept the political risks of public coalition with blacks? What were their hopes and fears, and where did they falter? And, finally, what does the substance of their effort tell us about the Democrats in the South and the Republicans in the North who opposed them?

Answers to these questions rest, in part, on detailed knowledge of such events as those in Grimes County, but they require more than compilations of local histories, just as they assuredly require more than cultural assessments based on novels, speeches, and party manifestoes considered apart from

⁴⁴ *Ibid.*, May 24, 1970.

⁴⁵ V. O. Key, *Southern Politics* (New York, 1949), is an authoritative study of the forms of Democratic orthodoxy in the various states of the old Confederacy, including the dominating orthodoxy of white supremacy; Vincent P. DeSantis, *Republicans Face the Southern Question* (Baltimore, 1959), summarizes the Republican failure to cope with the same imperatives.

their organic milieu. These answers will not provide much of a synthesis—Populism was too diverse, too congregational, and too ideologically thin—but they should tell us more about the larger society that, along with the Populists, failed to erect the foundations for a multiracial society in the nineteenth century. As the inquiry proceeds, it should be remembered that Populism perished before developing a mature philosophy—on race, on money, or on socialism. One must generalize, therefore, not only from contradictory evidence but, more important, from incomplete evidence. An analogy, doubtless unfair, could be made with the plight that would face modern historians of Marxism had that movement been abruptly truncated at the time, say, of the Brussels Conference in 1903. Who could have predicted on the evidence available to that date the Stalinist reign of terror that evolved from the mature, victorious revolutionary party of 1917? By the same token sweeping generalizations about what Populist radicalism could have become are not only romantic but historically unsound.

It should be sufficient to observe that in the long post-Reconstruction period—a period not yet ended—during which the social order has been organized hierarchically along racial lines, Populism intruded as a brief, flickering light in parts of the South. For a time some white Southerners threw off the romanticism that has historically been a cover for the region's pessimism and ventured a larger, more hopeful view about the possibilities of man in a free society. Under duress and intimidation this public hope failed of persuasion at the ballot box; under terrorism it vanished completely.

The Grimes County story dramatically illustrates this failure, but in the insight it provides into the underlying politics of black disfranchisement and the achievement of a monolithic one-party political environment in the American South it is not unique. Other Populists in East Texas and across the South—white as well as black—died during the terrorism that preceded formal disfranchisement. In Texas the extraparliamentary institutions formed by white Democrats to help create the political climate for disfranchisement bore a variety of local names: the Citizens White Primary of Marion County; the Tax-Payers Union of Brazoria County; the Jaybird Democratic Association of Fort Bend County; and the White Man's Union of Wharton, Washington, Austin, Matagorda, Grimes, and other counties.⁴⁶

⁴⁶ J. A. R. Moseley, "The Citizens White Primary of Marion County," *Southwestern Historical Quarterly*, 49 (1946): 524–31; Pauline Yelderman, "The Jaybird Democratic Association of Fort Bend County" (master's thesis, University of Texas, 1938); Millie L. Kochan, "The Jaybird-Woodpecker Feud: A Study in Social Conflict" (master's thesis, University of Texas, 1929); Ira Brandon, "The Tax Payers Union in Brazoria County," *Texas History Teachers Bulletin*, 14 (1926): 86–92. Roscoe Martin reflects a knowledge of these extraparliamentary institutions, though the closest the author comes to exploring the topic is the following footnote: "One who is willing to undergo the hardships involved may learn many interesting things concerning the White Man's Party from those who have a first hand knowledge of the organization. Practically nothing, however, has been written on the subject." *The People's Party in Texas* (2d ed.; Austin, 1970), 236n. Other than accounts reflecting the perspective of the founders of these institutions, the statement is as true in 1971 as when Martin wrote in 1933.

The available historical material concerning each of these organizations comes largely from the founders themselves, or their descendants, reflecting an incipient or a mature oral tradition—one oral tradition.⁴⁷ The secondary literature based on these accounts, including scholarly works used in graduate schools as well as primary and secondary textbooks, is correspondingly inadequate.⁴⁸

A surprising amount of uninterpreted material from violently partisan white supremacist sources has found its way into scholarly literature. One example from the Grimes experience pertains directly to the scholarly characterization of Negro political meetings during the Populist era. It is worth attention as an illustration of the impact of white supremacist modes of thought on modern scholarship. The sunup-to-sundown work routine of Southern farm labor obviously precluded daytime political meetings. Accordingly, Kennard, Haynes, and Carrington campaigned among their black constituents by holding political meetings in each of the towns and hamlets of the county at night. Democratic partisans termed these rallies "Owl Meetings" and characterized black Populist leaders as "'fluence men." Drawing upon their own party's time-honored campaign technique with Negroes, Democrats further asserted that owl meetings were more concerned with sumptuous banquets and whisky than with politics. If partisans of white supremacy had difficulty finding reasons for white acceptance of political coalition with blacks, they were culturally incapable of ascribing reasons for Negro support of the third party to causes other than short-run benefits in terms of money and alcohol. The point is not that Democrats were always insincere in their descriptions (as white supremacists they were quite sincere),

⁴⁷ J. A. R. Moseley is the son of the founder of the Marion County Citizens White Primary. Both the Yelderman and Kochan manuscripts on the Jaybird Democratic Association rest on versions supplied by founders, as does the Brandon article on Brazoria County. The following extract from Brandon may be taken as indicative of the style of this genre: "On the night before the returns were canvassed, a comparatively small band of determined, conservative, honest, white, Christian, representative men of the county assembled . . . and the result of their deliberations was the creation of the present Tax Payers' Union of Brazoria County. . . . According to the rules, only white men can be members of this union and . . . vote in the 'Tax Payers' Primary.'" "Tax Payers Union," 87. Douglas G. Perry makes no inquiry into the structure of the party at the local level, in Grimes or any other Texas county, nor does he investigate the politics of black disfranchisement as it affected the People's party. "Black Populism: The Negro in the People's Party" (master's thesis, Prairie View Agricultural and Mechanical College, 1945).

⁴⁸ Dewey Grantham, *The Democratic South* (Athens, Ga., 1963), is but one of the more recent manifestations of a long scholarly tradition in the South reflecting an unconscious assumption that reform politics is a function of white Southerners and that the observable victims of "Negrophobia" are Southern white progressives who are forced to employ race-baiting demagoguery in order to prevail at the polls. In this context see also Grantham's *Hoke Smith and the Politics of the New South* (Baton Rouge, 1958), 178: "The publicity given [Smith's] anti-Negro measures during the years 1905-1909 stamped him in the eyes of the nation as a Southern demagogue. It was unfortunate for his reputation as a Progressive leader that his work should have been marred in this respect." The failure of this kind of monoracial Southern scholarship rests less in its detail than in its underlying perspective on the qualifications for being "Southern" and the criteria upon which progressive "reputations" are based. Negroes lived desperately "political" lives during the period covered by Professor Grantham's books, though the substance of this politics, after disfranchisement, rarely took the form of decisions made at a ballot box.

but that scholars have subsequently accepted such violently partisan accounts at face value. The darkly sinister picture of “ ‘fluence men” corrupting innocent blacks with whisky at surreptitious owl meetings served to justify, at least to outsiders, the use of terrorism as the ultimate campaign technique of Democratic interracial politics. This sequential recording of events has found its way into scholarly monographs that otherwise demonstrate no inherent hostility to the Populistic inclinations of Southern farmers, black or white. In *The People’s Party in Texas* Roscoe Martin precedes his brief allusion to the White Man’s Union with a resumé of owl meetings and “ ‘fluence men” that reflects in detail the bias of white supremacist sources.⁴⁹ Other scholars writing broadly about Gilded Age politics have routinely drawn upon such monographs as Martin’s, and by this process “ ‘fluence men” have materialized as an explanation of Negro political insurgency in the nineties.⁵⁰ In the heat of local political combat, however, Democratic leaders often were able to face a wholly different set of facts in the course of persuading their followers, and the citizenry as a whole, to adjust to the necessity of terrorism. As the time approached for actual precinct campaigning in Grimes County in the autumn of 1900, the executive board of the White Man’s Union published a notice of the Union’s intentions, climaxed by a “fair distinct warning” to the county’s Negro leadership. The statement is revealing—not only of the transformation visited upon normal campaign practices when they were viewed through the cultural presumptions of white supremacy but also of the dangers of uncritical acceptance of such perspectives by scholars relying upon monoracial sources. The notice read in part:

The Union is largely composed of the best citizens of the county. . . . They are the tax payers, representing the worth, the patriotism, the intelligence, and the virtues of the county. . . . We are not fighting any political party or individuals, but only those who band together under any name, who seek to perpetuate negro rule in Grimes County. [Good citizens] are astounded at the manner in which the children’s money has been expended. Colored teachers with fat salaries and totally incompetent have been appointed for political “fluence.” Our white teachers, male and female, enjoy no such fat salaries as these colored politicians or these sweet colored girls. . . . One of the most corrupting practices in the past has been the system of Owl Meetings which has been in vogue for years. . . . This is the school and hot bed where the negro politician received his inspiration, and riding from one end of the county to the other as an apostle of his race, corrupting his own people who may be in the honest pathway of duty. We give fair warning that any effort to continue these Owl Meetings—by the appointment of special deputies sheriffs to organize and carry them on—will be prevented. No threat of shot-guns will deter us from the discharge of this duty.⁵¹

Even without recourse to other perspectives this view of the existing political situation in Grimes County contains serious internal contradic-

⁴⁹ Martin, *People’s Party* 179–83, 236.

⁵⁰ See, for example, H. Wayne Morgan, *From Hayes to McKinley* (New York, 1969), 382.

⁵¹ *Examiner*, Sept. 13, 1900. Jack Haynes was murdered two weeks after publication of this statement.

tions. Black Populist leaders were "incompetent" but as "apostles of their race" they had been so effective that their efforts needed to be stopped. Black teachers were paid "fat salaries" solely for political reasons, but among those receiving such gross patronage were "sweet colored girls," who obviously were not conducting owl meetings. The assertion that black teachers were actually paid more than white teachers must be rejected out of hand. In addition to the compelling fact that such an arrangement would have constituted poor political behavior on the part of a third party strenuously endeavoring to hold a substantial portion of the white vote and the further reality that such expenditures were unnecessary since parity for blacks in itself would have represented a notable accomplishment in the eyes of Negro leaders, Democrats had access to the records of all county expenditures and no such charge was ever leveled, much less documented, at any other time during the Populist decade. Whites complained that Negro teachers received "too much," not that they received more than white teachers. In any case, it seems necessary only to observe that American political parties have routinely utilized night gatherings without having their opponents characterize them as owl meetings and that persons who benefited from incumbency were not presumed to be acting in sinister ways when they campaigned for their party's re-election. The only thing "special" about Garrett Scott's deputies was that some of them were black. Viewed as some sort of black abstraction Jim Kennard might appear convincing as a shadowy "'fluence man," but as an intelligent and determined voice of the aspirations of Negro people he merits scholarly attention from perspectives not bounded by the horizons of those who murdered him. To an extent that is perhaps not fully appreciated, decades of monoracial scholarship in the South have left a number of Jim Kennards buried under stereotypes of one kind or another. They sometimes intrude anonymously as "'fluence men," but they simply do not appear as people in books on Southern politics.

This circumstance suggests that not only the broad topic of interracial life and tension but the entire Southern experience culminated by disfranchisement needs to be tested by a methodology that brings both black and white sources to bear on the admittedly intricate problem of interpreting a free society that was not free. At all events, evidence continues to mount that monoracial scholarship, Northern and Southern, has exhausted whatever merit it possessed as an instrument of investigating the variegated past of the American people. The obvious rejoinder—that written black sources do not exist in meaningful quantity—cannot, of course, be explained away; at the same time, this condition suggests the utility of fresh attempts to devise investigatory techniques that offer the possibility of extracting usable historical material from oral sources. The example of the erroneous report in the *Navasota Examiner* of Morris Carrington's death⁵² illustrates, perhaps as well as any single piece of evidence, not only the dangers inherent in

⁵² See note 26 above.

relying on such "primary sources" for details of interracial tension in the post-Reconstruction South but also the value of received oral traditions in correcting contemporary accounts. Nevertheless, the problem of evaluating such source material remains; white and black versions of the details of racial conflicts are wildly contradictory. When they are measured against other contemporary evidence, however, the interpretive problem becomes considerably less formidable; indeed, the task of penetrating the substance behind partisan contemporary accounts may be lessened through recourse to available oral sources, as I have attempted to demonstrate.

Since much of the *Realpolitik* of the South, from Reconstruction through the modern civil rights movement, rests on legal institutions that, in turn, rest on extralegal methods of intimidation, the sources of political reality may be found less in public debate than in the various forms of intimidation that matured in the region. However determined a historian may be to penetrate the legal forms to reach this extralegal underside of the political culture of the South he is, in our contemporary climate, blocked off from part of his sources by his skin color. For black scholars there are limits to the availability both of courthouse records in the rural South and of responsive white oral sources. There are corresponding limits to the information white scholars can gain from interviews in black communities. Here, then, is fertile ground for scholarly cooperation. Methods of achieving this cooperation need to be explored. In its fullest utilization the subject is not black history or Southern history but American history.

The Psychohistorical Origins of the Nazi Youth Cohort

PETER LOEWENBERG

A man has come into the world; his early years are spent without notice in the pleasures and activities of childhood. As he grows up, the world receives him when his manhood begins, and he enters into contact with his fellows. He is then studied for the first time, and it is imagined that the germ of the vices and the virtues of his maturer years is then formed.

This, if I am not mistaken, is a great error. We must begin higher up; we must watch the infant in his mother's arms; we must see the first images which the external world casts upon the dark mirror of his mind, the first occurrences that he witnesses; we must hear the first words which awaken the sleeping powers of thought, and stand by his earliest efforts if we would understand the prejudices, the habits, and the passions which will rule his life. The entire man is, so to speak, to be seen in the cradle of the child.

Alexis de Tocqueville, *Democracy in America* (1835)

Fate is not the sum of individual destinies any more than group formation can be comprehended as the mere coincidental coming together of numerous individuals. A sharing of the same world and the readiness to seize certain possibilities are the predeterminants that from the beginning guide Fate. The power of Fate is freed in the process of sharing and of conflict. The inescapable Fate of existence in and with one's "generation" completes the full density of individual existence.

Martin Heidegger, *Sein und Zeit* (1927)

La guerre—ce sont nos parents

Ernst Glaeser, *Jahrgang 1902* (1929)

THE HISTORICAL RELATIONSHIP between the events of World War I and its catastrophic aftermath in Central Europe and the rise of National Socialism has often been postulated. The causal relationship is usually drawn from the

The preparation of this essay has been facilitated by a study fellowship from the American Council of Learned Societies. I am especially indebted to the brilliant paper of Martin Wagh, "National Socialism and the Genocide of the Jews," *International Journal of Psychoanalysis*, 45 (1964): 386-95, to Peter Merkl, who generously shared unpublished research data with me, and to Gerhard Masur for his technical advice. For their critiques of earlier versions of this paper I am indebted to Robert G. L. Waite, William Niederland, Fred Weinstein, Franklin Mendels, Herbert Moller, Heiman van Dam, and to the members of the Interdisciplinary Colloquium on the Alternation of War and Peace of the American Psychoanalytic Association.

savagery of trench warfare on the western front, the bitterness of defeat and revolution, to the spectacular series of National Socialist electoral victories beginning in 1930, as if such a relationship were historically self-evident. It is the thesis of this paper that the relationship between the period from 1914 to 1920 and the rise and triumph of National Socialism from 1929 to 1935 is specifically generational. The war and postwar experiences of the small children and youth of World War I explicitly conditioned the nature and success of National Socialism. The new adults who became politically effective after 1929 and who filled the ranks of the SA and other paramilitary party organizations such as the Hitler-Jugend and the Bund-Deutscher-Madel were the children socialized in the First World War.

This essay examines what happened to the members of this generation in their decisive period of character development—particularly in early childhood—and studies their common experiences in childhood, in psychosexual development, and in political socialization that led to similar fixations and distortions of adult character. The specific factors that conditioned this generation include the prolonged absence of the parents, the return of the father in defeat, extreme hunger and privation, and a national defeat in war, which meant the loss of the prevailing political authority and left no viable replacement with which to identify.

Most explanations for the rise of National Socialism stress elements of continuity in German history. These explanations point to political, intellectual, social, diplomatic, military, and economic factors, all of which are important and none of which should be ignored. The historian and social scientist studying nazism should be conversant with and well versed in these categories of explanation. The study of political leadership is also of unquestioned importance for the understanding of the dynamics of totalitarianism, and it should be intensively developed by historians as an approach to that understanding.¹

This essay, however, will focus not on the leader but on the followers, not on the charismatic figure but rather on the masses who endow him with special superhuman qualities. It will apply psychoanalytic perceptions to the problem of National Socialism in German history in order to consider

¹ There is a growing bibliography of psychodynamic studies of Nazi leaders. See Gertrud M. Kurth, "The Jew and Adolf Hitler," *Psychoanalytic Quarterly*, 16 (1947): 11–32; Robert G. L. Waite, "Adolf Hitler's Guilt Feelings: A Problem in History and Psychology," *Journal of Interdisciplinary History*, 1 (1971): 229–49; and his "Adolf Hitler's Anti-Semitism: A Study in History and Psychoanalysis," in Benjamin B. Wolman, ed., *The Psychoanalytic Interpretation of History* (New York, 1971), 192–230; James H. McRandle, *The Track of the Wolf: Essays on National Socialism and Its Leader, Adolf Hitler* (Evanston, 1965); Peter Loewenberg, "The Unsuccessful Adolescence of Heinrich Himmler," *AHR*, 76 (1971): 612–41; Richard McMasters Hunt, "Joseph Goebbels: A Study of the Formation of His National-Socialist Consciousness" (Ph.D. dissertation, Harvard University, 1960); and Norbert Bromberg, "Totalitarian Ideology as a Defense Technique," *Psychoanalytic Study of Society*, 1 (1961): 26–38. Sigmund Neumann has treated the Nazi leadership, as distinguished from the mass of party adherents, in terms of the generation born between 1890 and 1900 in *The Future in Perspective* (New York, 1946), 221–25.

the issues of change rather than continuity in history, to deal with social groups rather than individual biography, and to focus on the ego-psychological processes of adaptation to the historical, political, and socioeconomic context rather than on the instinctual biological drives that all men share.²

The rapid political ascendancy of the NSDAP in the period from 1928 to 1933 was marked by particularly strong support from youth. Since this generation experienced childhood deprivation in World War I, the argument becomes a psychoanalytical one of taking seriously the developments of infancy and childhood and their effect on behavior in adulthood. I wish to offer an added factor, one to be included as an explanation in addition to rather than instead of the other explanatory schemata of history. Both history and psychoanalysis subscribe to overdetermination in causation. It would be a poor historian who sought to attribute a war or a revolution to only a single cause. Similarly in psychoanalytic theory every symptom and symbol is psychically overdetermined and serves multiple functions. When the subject of study is a modern totalitarian mass movement it requires analysis utilizing all the tools for perceiving and conceptualizing irrational and affective behavior that the twentieth century has to offer, including psychoanalysis and dynamic psychology.³

No genuine historical understanding is possible without the perspective of self-understanding from which the historian can then move forth to deal with historical materials. Likewise there can be no measure of historical understanding if we research what men said and did and fail to understand why they acted. The twentieth century has experienced the gross magnification of political and personal irrationality correlative to the exponential increment in the power of modern technology.⁴ No history will speak with relevance or accuracy to the contemporary human condition if it

² History is an unceasing dialectic between the forces of continuity and change. Most psychoanalytical approaches to history have stressed the proposition of continuity, focusing on the timeless and unconscious id qualities of sexuality and aggression. See Herbert Marcuse, *Eros and Civilization: A Philosophical Inquiry into Freud* (Boston, 1956); Norman O. Brown, *Life against Death: The Psychoanalytical Meaning of History* (Middletown, Conn., 1959); and Brigid Brophy, *Black Ship to Hell* (New York, 1962). For a work that incorporates concepts of ego adaptation to historical changes, see Fred Weinstein and Gerald M. Platt, *The Wish to be Free: Society, Psyche, and Value Change* (Berkeley, 1969). Where psychoanalysis is applied to biography, the emphasis is usually placed appropriately on the elements of continuity in personality patterns from childhood to maturity. See, for example, Alexander L. and Juliette L. George, *Woodrow Wilson and Colonel House* (New York, 1956); Fawn M. Brodie, *Thaddeus Stevens: Scourge of the South* (New York, 1959); and Rudolph Binion, *Frau Lou: Nietzsche's Wayward Disciple* (Princeton, 1968).

³ "Dynamic" in psychology is a descriptive term used to imply activity in contrast to theories that are "static" or "potential." Karl Menninger defines dynamic psychology and psychoanalysis as "theories of personality in which motivation, especially unconscious, is considered basic." *The Vital Balance: The Life Process in Mental Health and Illness* (New York, 1963), 467. Henry Murray uses dynamic "to designate a psychology which accepts as prevailingly fundamental the goal directed (adaptive) character of behavior, and attempts to discover and formulate the internal as well as the external factors which determine it." As quoted in *ibid.*

⁴ This essentially Marxist point has been most elegantly developed by Herbert Marcuse in *Eros and Civilization* (Boston, 1955) and in his *One-Dimensional Man* (Boston, 1964).

fails to assess realistically the profound capacity of the irrational to move men.

Psychoanalysts are concerned with many things that are relevant to the historical problem of what happens to children in a nation at war. They have studied the effects of separation from parents and have seen the long-term consequences of deprivation, material and emotional. They know the hows and whys of a child's identification with his parents. Above all, psychoanalysis as a clinical technique of investigation demonstrates that only the smallest part of human thought and conduct is rational. The world of disembodied minds acting in an emotional vacuum has no place in a psychoanalytically informed history. Too much of history is still written as though men had no feelings, no childhood, and no bodily senses. What is needed is a new kind of history, a history that tells us how men responded to and felt about the great political and economic events that shaped their lives, a history that gives due place to the irrational, the unconscious, and the emotions not only of men, but also of the child in the man.⁵

This new kind of history requires an understanding of the dual and related concepts of fixation and regression. Sigmund Freud, in a demographic metaphor of migration, once compared human development to the progress of a people through new territory. At those points where resistance is greatest and conflict most intense the people will leave behind its strongest detachments and move on. If the advanced parties, now reduced in strength, should suffer defeat or come up against a superior enemy, they will retreat to former stopping places where support stands ready. "But," says Freud, "they will also be in greater danger of being defeated the more of their number they have left behind on their migration." Thus, the greater the strength of early fixations, the greater will be the later need for regression: "The stronger the fixations on its path of development, the more readily will the function evade external difficulties by regressing to the fixations—the more incapable, therefore, does the developed function turn out to be of resisting external obstacles in its course."⁶ As in Freud's migration metaphor, when an individual who has passed through the maturational phases of development meets with persistent and intense frustration, one of the means

⁵ Psychoanalysis is distinguished from other psychologies in that it treats with seriousness the psychological developments of infancy and childhood. The dynamic relationship between the fantasies and behavior of childhood and "rational" adult patterns of conduct constitutes the special interest of psychoanalysis for the historian. Clinical evidence for this relationship can be demonstrated in the common features that have been established between early childhood mentation, the world of dreams in normal adults, and the thought of neurotics and psychotics. Another line of continuity between child and adult has been established by the social sciences in their study of the childhood socialization process. I will use both methodological approaches in this essay, for they are complementary.

⁶ Sigmund Freud, "Introductory Lectures on Psycho-analysis" (1916–17), reprinted in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, tr. and ed. James Strachey et al. (London, 1953–), 16: 341. I am indebted to Martin L. Grotjahn and Lilla Veszy-Wagner for aid in locating this citation.

of coping with the pain and lack of satisfaction is to revert from the more highly developed stages of mental organization to modes of functioning typical of an earlier period.⁷ The falling back, or regression, will be to phases of psychosexual development that have left areas of weakness, where the maturational step has been marked by unresolved conflicts and anxieties. Arrests of development or points of fixation occur in sexual-drive organization, ways of relating to people, fears of conscience, persistence of primitive kinds of gratification and of reacting defensively to old, no longer present, dangers. As Freud formulated it in 1913:

We have become aware that the psychical functions concerned—above all, the sexual function, but various important ego functions too—have to undergo a long and complicated development before reaching the state characteristic of the normal adult. We can assume that these developments are not always so smoothly carried out that the total function passes through this regular progressive modification. Wherever a portion of it clings to a previous stage, what is known as a “point of fixation” results, to which the function may regress if the subject falls ill through some external disturbance.⁸

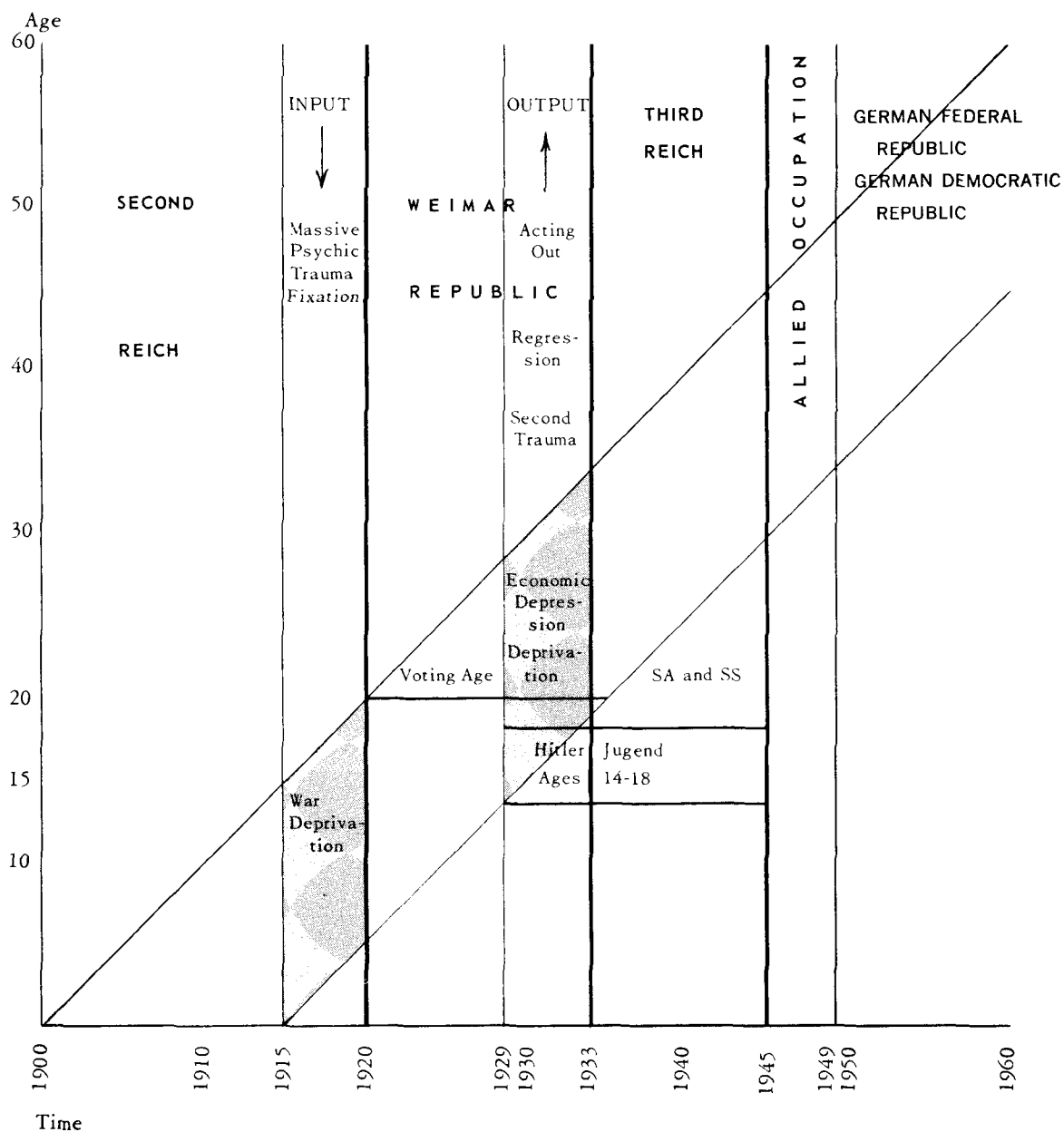
The concepts of fixation and regression may be best illustrated by an operational example taken from a clinical case. A German lady comes into psychoanalytic treatment because of intense marital discord and an acute telephone phobia that interferes with her work. She cannot speak on the telephone, breaks out into a cold sweat, becomes intensely anxious, and loses her voice. In 1943, when she was three years old, she experienced the bombing of Hamburg. She remembers the air raids, the burning and explosions. She was not evacuated. Her family lived near the city center. Her father was a fireman who was called to duty by a bell that rang on the wall of the house because the family had no telephone. The patient can recall being strafed by an airplane. She has no recollection, however, of any panic, fear, or rage. Her memories are affectless. They are clear but disassociated from any of the powerful emotions that must have been present in the child. Now, in a current marital crisis, her feelings of explosive destructive anger and fears of abandonment by a man who is important to her cause a regression. The symptom of the telephone bell symbolizes an earlier point of fixation when she was traumatized by fears of external disaster and internal loss. She now, as an adult, re-experiences all of the emotions that were buried and repressed after the childhood trauma because the later, adult trauma has mobilized the earlier point of fixation and caused a regression to the feelings of the child.

Returning to the larger historical case of the German children of the First World War, it is Germany's Great Depression, with its unemployment, governmental chaos and impotence, and widespread anxiety about the future

⁷ Sigmund Freud, “Types of Onset of Neurosis” (1912), in *ibid.*, 12: 232.

⁸ Sigmund Freud, “The Disposition to Obsessional Neurosis: A Contribution to the Problem of Choice of Neurosis” (1913), in *ibid.*, 317–18.

BIRTH COHORTS 1900 – 1915 IN GERMAN HISTORY



that constituted precisely such an “external disturbance” as Freud describes. The early point of fixation was the First World War, when the peoples of Central Europe experienced prolonged hunger, war propaganda, the absence of fathers and often both parents, and the bankruptcy of all political values and norms.

The psychological symptoms of regression to phases of ego functioning “fixed” by the traumata of a childhood in war included responding to internal personal stress with externalized violence, projecting all negative anti-national or antisocial qualities onto foreign and ethnic individuals and groups, and meeting frustrations that would otherwise be tolerated with patience and rationally approached for solutions with a necessity for immediate gratification.⁹ The political expression of weakened egos and superegos that fostered regression was manifest not only in turning to violence but most especially in the longing for a glorified and idealized but distant father who is all-knowing and all-powerful, who preaches the military virtues and permits his sons and daughters to identify with him by wearing a uniform and joining combat in a national cause.

IT IS TIME TO LAY AT REST the idea that psychoanalytical explanations are necessarily unicausal or that they are inherently incompatible with quantitative data such as demographic, election, consumption, and health statistics. Indeed, psychoanalysis can give these macrodata new coherence and meaning, thus adding a vital qualitative dimension to history. Psychohistory uses dynamic psychology to integrate political and economic explanations with past experience, patterns of repetition, and the irrationality of conduct in times of anxiety, deprivation, and stress.

Traditional psychological interpretations of both political leadership and the personal dynamics of the adherents of mass totalitarian movements, in their explanatory model of adult political behavior, have stressed origins in childhood emotional traumata and in relations with the parents. This has been a particularly successful approach with biography. A consideration of childhood certainly reveals much about the way people are programmed to respond in adulthood. Yet intensive experiences in later life, if they are of a massive traumatic nature, can supersede both earlier influences and individual predispositions. This means that a major catastrophe will have an impact on all ages who are subject to its blows. It will necessarily affect the very young most because their egos are the most fragile. But it will also affect children in latency and adolescence and even adults, each according to his ego strength—that is, according to his ability to tolerate frustration, anxiety, and deprivation. In other words, if the adult trauma is great enough, for example an economic depression or a lost war, it does not matter who the parents were or how democratic they may have been; the anxiety-

⁹ For a clinical discussion of the low frustration tolerance due to weakened egos that leads to aggression, see Fritz Redl and David Wineman, *The Aggressive Child* (New York, 1957), 76–78.

inducing social or political situation will bring to the fore feelings of helplessness and political irrationality. One of the foremost students of the authoritarian personality, Else Frenkel-Brunswik, has pointed out that "it seems that external pressures of a traumatic character, be they past or be they presently imposed, are likely not only to bring authoritarian personalities to the fore but to reinforce authoritarian trends in individuals who otherwise would remain democratic minded."¹⁰

The demographic approach offers new categories of explanation and presents an advantage from the standpoint of evidence. Human motivation and behavior is infinitely complex. Any choice of action by a single individual may be attributed to a multiplicity of unique and idiosyncratic causes that could be clarified only after an extensive psychoanalysis. The appeal of a generational approach is that it deals with probabilities—with the law of averages on a macroscale—thus canceling out any of the many individual variables that determine conduct. Whereas it can always be said that in a particular case there are other variables that have been overlooked, such an objection does not hold when we deal with a demographic scale of events affecting a population. In the latter case we have responses of an entire society to events that, while they may be confirmed in many particular cases, are not limited in their general impact by the idiosyncratic developments of a single life.

The seminal conceptual formulation of the generation as a force acting in history was established by Karl Mannheim in 1927 in his essay, "The Sociological Problem of Generations."¹¹ Here Mannheim speaks of the human mind as "stratified" or layered, with the earliest experiences being the basis, and all subsequent experience building on this primary foundation or reacting against it. The influence of psychoanalytic thought on Mannheim's conceptualization of the problem is apparent.

The human consciousness, structurally speaking, is characterized by a particular inner "dialectic." It is of considerable importance for the formation of the consciousness which experiences happen to make those all-important "first impressions," "childhood experiences"—and which follow to form the second, third, and other "strata." Conversely, in estimating the biographical significance of a particular experience, it is important to know whether it is undergone by an individual as a decisive childhood experience, or later in life, superimposed upon other basic and early impressions. Early impressions tend to coalesce into a *natural view* of the world. All later experiences then tend to receive their meaning from this original set, whether they appear as that set's verification and fulfillment or as its negation and antithesis. . . . Mental data are of sociological importance not only because of their actual content, but also because they cause the individuals sharing them to form one group—they have a socializing effect.¹²

¹⁰ Else Frenkel-Brunswik, "Environmental Controls and the Impoverishment of Thought," in Carl J. Friedrich, ed., *Totalitarianism* (New York, 1964), 177.

¹¹ Karl Mannheim, "The Sociological Problem of Generations," in Paul Kecskemeti, ed., *Essays on the Sociology of Knowledge* (London, 1952), 276–320.

¹² *Ibid.*, 298, 304.

Mannheim then structures a further "concrete nexus" of the generation in history as "*participation in the common destiny* of [the] historical and social unit." And such groups he terms "generation units."

Youth experiencing the same concrete historical problems may be said to be part of the same actual generation while those groups within the same actual generation which work up the material of their common experiences in different specific ways, constitute generation units. . . . These are characterized by the fact that they do not merely involve a loose participation by a number of individuals in a pattern of events shared by all alike though interpreted by the different individuals differently, but an identity of responses, a certain affinity in the way in which all move with and are formed by their common experiences.¹³

This means that those of a generation who experienced the same event, such as a world war, may respond to it differently. They were all decisively influenced by it but not in the same way. Some became pacifists, others embraced international Leninism, some longed to return to the prewar, conservative, monarchist social order, and the ones we are concerned with sought personal and national solutions in a violence-oriented movement subservient to the will of a total leader. What was politically significant in the early 1930s was the facility with which individuals of this generation moved from one allegiance to the other. Mannheim's point is that although the units of a generation do not respond to a formative crisis in the same way due to a multiplicity of variables, the overriding fact is their response to that particular event. Because of this they are oriented toward each other for the rest of their lives and constitute a generation.

An organization, such as a youth group, says Mannheim, may serve to mobilize latent opinion in a generation unit. It attracts to itself those individuals who share the formative experiences and impulses of the particular generation location, thus institutionalizing and realizing collectively the potentialities inherent in the historical and social situation.¹⁴

Following the theoretical work of Mannheim, sociological demographers have developed the highly suggestive concept of the "cohort," a term whose Latin etymology significantly refers to a group of fighting men who made up one of the ten divisions of a legion in the Roman army. In the modern discipline of demography a cohort is the aggregate of individuals within a population who have shared a significant common experience of a personal or historical event at the same time. This is distinguished from the loose term "generation," by which historians usually mean a temporal unit of family kinship structure such as "the founding generation," or, more ambiguously, a broad and often unspecified age span during a particular institutional, political, or cultural epoch, such as "the generation of '48" or "the lost generation." An example of a cohort would be college graduates of the year 1929, who completed their education in prosperity and in their

¹³ *Ibid.*, 303, 304, 306; italics in original.

¹⁴ *Ibid.*, 310.

first months on the labor market experienced the onset of the Great Depression. This cohort is distinctively marked by the period-specific stimulus of the economic depression for their entire working years in the labor force so that they are to be distinguished from other cohorts, even thirty years later, by their common experience of having endured significant events simultaneously. The same may be said for those who served in the armed forces during World Wars I and II, or those who were children during a war.

These are, of course, examples of birth cohorts. But a cohort need not necessarily be born at the same time. A cohort may include people of all ages, even those *in utero*, if the historian seeks to define all of those who were influenced by a single traumatic event. When Robert Jay Lifton, for example, studied the people who were victims of the atomic bombing of Hiroshima, he interviewed a wide range of people—from a boy who was but two years old to a number of elderly men and women—who constitute the cohort of atomic survivors.¹⁵ Likewise those who have survived a Nazi concentration camp, regardless of age, have been through a traumatic experience that marks them for life.¹⁶ They will never be the same; they are the concentration-camp cohort. As the demographer Norman Ryder says: "The concept [of cohort analysis] can be extended to the identification and surveillance of any group in terms of the time it enters any category of exposure to an event or behavior pattern of interest."¹⁷ Thus each cohort is itself unique; its members are different from all those who have preceded it and all who will follow because they have experienced certain traumatic episodes in their collective life at a common time and a specific historical moment.

In emphasizing what distinguishes one generation from another, Ryder and other generation and cohort theorists naturally tend to understate the equally important bonds of continuity that tie a society together by connecting one generation to another. In the last analysis it is these latter attachments that are most fundamental to society because they provide for the transmission of cultural modes such as language and social norms of behavior from parents to children and thus from one generation to another.¹⁸

¹⁵ Robert Jay Lifton, *Death in Life: Survivors of Hiroshima* (New York, 1967).

¹⁶ See Henry Krystal, ed., *Massive Psychic Trauma* (New York, 1968); Klaus D. Hoppe *et al.*, "The Emotional Reactions of Psychiatrists when Confronting Survivors of Persecution," *The Psychoanalytic Forum*, 3 (1969): 185–211; Elie A. Cohen, *Human Behavior in the Concentration Camp*, tr. M. H. Braaksma (New York, 1953); Bruno Bettelheim, *The Informed Heart: Autonomy in a Mass Age* (Glencoe, Ill., 1960), 107–235; Elie Wiesel, *Night*, tr. Stella Rodway (New York, 1961); and Primo Levi, *Survival in Auschwitz: The Nazi Assault on Humanity*, tr. Stuart Woolf (New York, 1959).

¹⁷ Norman B. Ryder, "Cohort Analysis," *International Encyclopedia of the Social Sciences* (2d ed.; New York, 1968), 2: 549.

¹⁸ On the integration of the child into the social system during the first few weeks of life, see Talcott Parsons, "Social Structure and the Development of Personality: Freud's Contribution to the Integration of Psychology and Sociology," in his *Social Structure and Personality* (New York, 1964), 78–111. The process of the birth of the individual personality is described by Margaret Mahler as follows: "Infants present a large variety of cues—to indicate needs, tension, and pleasure. In a complex manner, the mother responds selectively to only *certain* of these

War has received special attention from cohort theorists as the most dramatic instance of a cohort being influenced by external events. Ryder writes

The Great War weakened a whole cohort in Europe to the extent that normal succession of personnel in roles, including positions of power, was disturbed. Sometimes the old retained power too long; sometimes the young seized power too soon. The most obvious effect of war is the mortality and morbidity of the participants, but war transforms non-combatants as well. . . . Traumatic episodes like war and revolution may become the foci of crystallization of the mentality of a cohort. The dramatic impact may mark indelibly the "naive eyes and virgin senses" of the cohort . . . and change them into . . . a virtual community of thought and action. . . . Solidarity is encouraged by idealized self-definitions, . . . by sharing anxieties concerning imminent and hazardous transitions, and by explicit associations that encourage the development of attitudes unsanctioned by family and community.¹⁹

The concept of the birth cohort—that is, those born at the same time—implies common characteristics because of common formative experiences that condition later life. Character formation, the direction of primary drives, and the internalization of family and social values are determined in the years of infancy and childhood. Each cohort carries the impress of its specific encounter with history, be it war or revolution, defeat or national disaster, inflation or depression, throughout its life.²⁰ Any given political, social, or economic event affects people of different ages in different ways. The impact of war, hunger, defeat, and revolution on a child will be of an entirely different order of magnitude than the impact on an adult. This commonplace fact suggests that the event specificity of history must be fused

cues. The infant gradually alters his behavior in relation to this selective response; he does so in a characteristic way—the resultant of his own innate endowment and the mother-child relationship." *On Human Symbiosis and the Vicissitudes of Individuation*, vol. 1: *Infantile Psychosis* (New York, 1968), 18. This is not only how a child acquires his individual uniqueness; it is also the pattern of transmission and acquisition of the cultural norms of a society.

¹⁹ Norman B. Ryder, "The Cohort as a Concept in the Study of Social Change," *American Sociological Review*, 30 (1965): 848-49, 851, 855.

²⁰ Such cohort analysis, emphasizing the importance of time-specific childhood socialization, has enabled Ronald Inglehart, for example, to explain convincingly why Dutch adults favor European unification to a higher degree than the adults of France, Germany, or Britain. Inglehart attributes this to the fact that alone among these four countries, the Netherlands was not involved in World War I and the great-power struggles that preceded it, when the age group he tested in 1963 at age fifty-five and over would have been children. He suggests that this difference is due to "a residue from the experiences of childhood and youth" in which the individuals over fifty-five in France, Germany, and Britain "were exposed to the period of intense nationalism which preceded that war, and to the powerful fears and suspicions the war aroused during a relative[ly] impressionable stage of life." Inglehart goes on to postulate that because by the end of the 1970s a majority of the voting population in the Common Market countries will consist of people who entered primary school after World War II, thus having derived an early "sense of positive participation in common activities" and of growing up "with some awareness of common endeavor," the advocates of the movement for European integration will move into positions of leadership within their respective countries. "An End to European Integration?" *American Political Science Review*, 61 (1967): 91-105. The quotations are from pages 93 and 94. I am indebted to Professor Inglehart for sharing with me the manuscript of his article "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies" (to be published in *ibid.*, Dec. 1971), which applies cohort theory to European post-World War II intergenerational value conflict.

with the generational-age specificity of the cohort of sociological demography and the developmental-phase specificity of psychoanalysis and childhood socialization to understand historical change. In this sense history may be the syncretic catalyst of qualitative longitudinal life history and the quantitative data of sociological statistical analysis.²¹

RATHER THAN PROCEEDING with the story of the Nazi youth cohort chronologically and beginning with its origins, this essay will use what Marc Bloch termed the "prudently retrogressive" method of looking at the outcome first, and then tracking down the beginnings or "causes" of the phenomenon.²² This, of course, corresponds to the clinical method of examining the "presenting complaints" first and then investigating etiology. The outcome of the story in this case is the related and concomitant economic depression, the influx of German youth to the ranks of National Socialism, the political decline of the Weimar Republic, and the Nazi seizure of power.

The Great Depression hit Germany harder than any other country, with the possible exception of the United States. Germany's gross national income, which rose by 25 per cent between 1925 and 1928, sank 43 per cent from 71 billion RM in 1929 to 41 billion RM in 1932. The production index for industry in 1927-28 was halved by 1932-33. In the critical area of capital goods, production in 1933 was one-third of what it had been five years earlier. The very aspect of Nazi success at the polls in the elections of 1930 accelerated the withdrawal of foreign capital from Germany, thus deepening the financial crisis.

The greatest social impact of the economic crisis was in creating unemployment. By 1932 one of every three Germans in the labor market was without a job. This meant that even those who held jobs were insecure, for there were numerous workers available to take the place of every employee. The young people were, of course, the most vulnerable sector of the labor market. New jobs were nonexistent, and the young had the least seniority and experience with which to compete for employment. To this must be added that the number of apprenticeships was sharply diminishing for working-class youths. For example, apprenticeships in iron, steel, and metalworking declined from 132,000 in 1925 to 19,000 in 1932.²³ University graduates had no better prospects for finding employment. They soon formed an underemployed intellectual proletariat that looked to National Socialism for relief and status.

The electoral ascendancy of the Nazi party in the four years between 1928 and 1932 constitutes one of the most dramatic increments of votes and

²¹ For an excellent review of recent scholarship in historical demography, see Franklin F. Mendels, "Recent Research in European Historical Demography," *AHR*, 75 (1970): 1065-73.

²² Marc Bloch, *The Historian's Craft*, tr. Peter Putnam (New York, 1953), 45-46.

²³ Dieter Petzina, "Germany and the Great Depression," *Journal of Contemporary History*, 4 (1969): 59-74.

political power in the history of electoral democracy. In the Reichstag elections of May 20, 1928, the National Socialists received 810,127 votes, constituting 2.6 per cent of the total vote and 12 Reichstag seats. In the communal elections of 1929 the Nazis made decisive gains. With this election Germany had its first Nazi minister in Thuringia in the person of Wilhelm Frick, a putschist of 1923. In the next Reichstag elections of September 14, 1930, the National Socialists obtained 6,379,672 votes, for 18.3 per cent of the total and 107 seats. At the election of July 31, 1932, the National Socialists became the largest party in the country and in the Reichstag with 13,765,781 votes, giving them 37.4 per cent of the total vote and 230 parliamentary seats.²⁴

This extremely rapid growth of Nazi power can be attributed to the participation in politics of previously inactive people and of those who were newly enfranchised because they had reached voting eligibility at 20 years of age. There were 5.7 million new voters in 1930.²⁵ The participation of eligible voters in elections increased from 74.6 per cent in 1928 to 81.41 per cent in 1930, and 83.9 per cent in 1932. In the elections of March 5, 1933, there were 2.5 million new voters over the previous year and voting participation rose to 88.04 per cent of the electorate.²⁶

The German political sociologist, Heinrich Streifler, makes the point that not only were new, youthful voters added at each election, but there were losses from the voting rolls due to deaths that must be calculated. He shows that 3 million voters died in the period between 1928 and 1933. The increment of first-time, new voters in the same period was 6,500,000.²⁷

In the elections of 1928, 3.5 million young voters who were eligible did not participate in the voting. "This," says Streifler, "is a reserve that could be mobilized to a much greater extent than the older nonvoters."²⁸ He goes on to suggest that these young nonvoters were more likely to be mobilized by a radical party that appealed to passions and emotions than to reason.

The Nazis made a spectacular and highly successful appeal to German youth. An official slogan of the party ran "National Socialism is the organized will of youth" (*Nationalsozialismus ist organisierter Jugendwille*). Nazi propagandists like Gregor Strasser skillfully utilized the theme of the battle of the generations. "Step down, you old ones!" (*Macht Platz, ihr Alten!*) he

²⁴ The Nazi vote declined to 11,737,000, or 33.1 per cent in the elections of November 6, 1932. At the last quasi-free election in Germany, on March 5, 1933, five weeks after Hitler's accession to power, the Nazi vote was 17,277,200 or 43.9 per cent. See Koppel S. Pinson, *Modern Germany: Its History and Civilization* (2d ed.; New York, 1966), 603-04.

²⁵ I derived this figure by subtracting the total number of votes cast in 1928 (30,753,300) from the corresponding figure for 1930 (34,970,900), and adding the 1.5 million older voters who died in this period according to Arthur Dix, *Die Deutschen Reichstagswahlen 1871-1930 und die Wandlungen der Volksgliederung* (Tübingen, 1930), 36.

²⁶ Pinson, *Modern Germany*, 603-04.

²⁷ Heinrich Streifler, *Deutsche Wahlen in Bildern und Zahlen: Eine soziografische Studie über die Reichstagswahlen der Weimarer Republik* (Düsseldorf, 1946), 16.

²⁸ *Ibid.*, 20.

shouted as he invoked the names of the senior political leaders from Left to Right and associated them with the disappointments of the generation of the fathers and the deprivations of war, defeat, and revolution.

Whether they are named Scheidemann and Wels, whether Dernburg or Koch, whether Bell and Marx, Stresemann and Riesser, whether Hergt and Westarp—they are the same men we know from the time before the war, when they failed to recognize the essentials of life for the German people; we know them from the war years, when they failed in the will to leadership and victory; we know them from the years of revolution, when they failed in character as well as in ability, in the need of an heroic hour, which, if it had found great men, would have been a great hour for the German people—who, however, became small and mean because its leading men were small and mean.²⁹

The Nazis developed a strong following among the students, making headway in the universities in advance of their general electoral successes. National Socialism made its first visible breakthrough into a mass sector of the German people with its conquest of academic youth. The student government (ASTA) elections of 1929 were called a “National Socialist storm of the universities” by the alarmed opposition press. The Nazi Student Organization (Nationalsozialistische Deutsche Studentenbund) received more than half the votes and dominated the student government in 1929 at the universities of Erlangen and Greifswald. In the 1930 student election it also captured absolute majorities in the universities of Breslau, Giessen, Rostock, Jena, Königsberg, and the Berlin Technische Hochschule. Both of these student elections preceded the Reichstag elections of 1930 in which the Nazis made their decisive breakthrough into the center of national political life. Developments toward National Socialism among the university students anticipated by four years the developments in German society at large.³⁰

The comparative age structure of the Nazi movement also tells a story of youthful preponderance on the extreme Right. According to the Reich's census of 1933, those 18 to 30 constituted 31.1 per cent of the German population. The proportion of National Socialist party members of this age group rose from 37.6 per cent in 1931 to 42.2 per cent a year later, on the eve of power. “The National Socialist party,” says the sociologist Hans Gerth, “could truthfully boast of being a ‘young party.’” By contrast, the Social Democratic party, second in size and the strongest democratic force in German politics, had only 19.3 per cent of its members in the 18 to 30 age group in 1931.³¹ In 1930 the Social Democrats reported that less than 8

²⁹ Gregor Strasser, “Macht Platz, Ihr Alten!” speech delivered May 8, 1927, as quoted in Karl Dietrich Bracher, *Die Auflösung der Weimarer Republik: Eine Studie zum Problem des Machtverfalls in der Demokratie* (3d ed.; Villingen, Schwarzwald, 1960), 116 n.84.

³⁰ Karl Dietrich Bracher, *Die deutsche Diktatur: Entstehung, Struktur, Folgen des Nationalsozialismus* (Cologne, 1969), 179–83; Bracher, *Die Auflösung der Weimarer Republik*, 147–49; Wolfgang Zorn, “Student Politics in the Weimar Republic,” *Journal of Contemporary History*, 5 (1970): 128–43.

³¹ Hans H. Gerth, “The Nazi Party: Its Leadership and Composition,” *American Journal of Sociology*, 45 (1940): 529.

per cent of their membership was under 25, and less than half was under 40.³²

"National Socialism," says Walter Laqueur, the historian of the German youth movement, "came to power as the party of youth."³³ The Nazi party's ideology and organization coincided with those of the elitist and antidemocratic elements of the German youth movement. The Wandervogel, while essentially nonpolitical, retreated to a rustic life on the moors, heaths, and forests where they cultivated the bonds of group life. The Nazi emphasis on a mystical union of blood and soil, of *Volk*, nation, language, and culture, appealed to the romanticism of German youth *Bünde*.

The Hitler Youth adopted many of the symbols and much of the content of the German youth movement.³⁴ The Nazis incorporated the uniform, the Führer principle and authoritarian organization (group, tribe, *gau*), the flags and banners, the songs, and the war games of the *Bünde*.³⁵ The National Socialists were able to take over the youth movement with virtually no opposition. On April 15, 1933, the executive of the Grossdeutsche Jugendbund voted to integrate with the Nazi movement. On June 17, 1933, the Jugendbund was dissolved and Baldur von Schirach was appointed the supreme youth leader by Hitler.³⁶

A number of scholars have interpreted the radicalization of newly enfranchised German youth in the years of the rise of National Socialism. The Nazification of the youth has also been variously attributed to the spirit of adventure and idealism,³⁷ a lust for violence and military discipline,³⁸ the appeal of an attack on age and established power,³⁹ and the quest for emotional and material security.⁴⁰

Among the first and most incisive political analysts to focus on the youthful element in the success of National Socialism was the Left socialist leader Carl Mierendorff, who has been described by Koppel Pinson as "a flash of genius shining across the Socialist horizon" of the late Weimar years.⁴¹ After the municipal elections of November 1929, in which the Nazis made their first significant gains, Mierendorff called attention to the Nazi achievement of rivaling the Social Democratic party in breadth and scope of party

³² Peter Gay, *Weimar Culture: The Outsider as Insider* (New York, 1968), 140.

³³ Walter Z. Laqueur, *Young Germany: A History of the German Youth Movement* (London, 1962), 191.

³⁴ Wilhelm Flitner, "Der Krieg und die Jugend," in Otto Baumgarten *et al.*, eds., *Geistige und Sittliche Wirkungen des Krieges in Deutschland* (Stuttgart, 1927), 292-302, 346-56.

³⁵ Laqueur, *Young Germany*, 194; Bracher, *Die Auflösung der Weimarer Republik*, 131-32.

³⁶ Laqueur, *Young Germany*, 200-02.

³⁷ Reinhard Bendix, "Social Stratification and Political Power," *American Political Science Review*, 46 (1952): 357-75; Bracher, *Die Auflösung der Weimarer Republik*, 116.

³⁸ Petzina, "Germany and the Great Depression," 73; Dix, *Die Deutschen Reichstagswahlen*, 37-43.

³⁹ Alice Hamilton, "The Youth Who Are Hitler's Strength: A Study of the Nazi Followers and The Appeal That Has Aroused Them," *New York Times Magazine*, Oct. 8, 1933, pp. 3, 16.

⁴⁰ Rudolf Heberle, *From Democracy to Nazism: A Regional Case Study on Political Parties in Germany* (Baton Rouge, 1945), 9-10.

⁴¹ Pinson, *Modern Germany*, 415.

organization to the point where it now presented a challenge to the Socialists in every precinct and township. To explain this Mierendorff pointed to the age structure of the Nazi party. To a great extent National Socialist membership was born in the years between 1905 and 1912, which he termed: "a generation which knows little or nothing of the war." While Mierendorff's observations and data are excellent, the conclusion that children are ignorant of war and that, as a childhood experience, war will not affect them in later life, is a viewpoint that anyone conversant with modern concepts of psychology and the childhood socialization process is unlikely to share. The thesis propounded in this essay is in specific contravention to Mierendorff's Marxist interpretation. By contrast with his emphasis on conscious experience Mierendorff's psychological insight is perceptive when he evaluates the motives for this National Socialist appeal to youth. "It makes no intellectual demands of its followers, instead it expects of them first of all enthusiasm and both personal and intellectual arrogance. It flirts with pseudomascuine manners and presents itself in a basically aggressive pseudo-heroic posture."⁴²

The historical demographer Herbert Moller, on the other hand, stresses the factor of cohort size in creating the preconditions for political turbulence in Germany in the early 1930s. He points out that the proportion of young adults in Germany was very high at this time as a result of the high birth rates twenty to thirty years earlier. "The cohorts of 1900 to 1914," he writes, "more numerous than any earlier ones, had not been decimated by the war." Moller shows that precisely this cohort had its ranks swelled by immigrants from the territories ceded under the Treaty of Versailles and by German nationals from abroad, especially from Eastern and Southeastern Europe. Close to one and a half million immigrants entered Germany from 1918 to 1925, just when emigration to America was being curtailed by immigration-quota legislation in the United States. By 1930, because of the depression, a reverse movement of re-emigration from America back to Germany occurred. As a result of these developments, in 1933 the age group from 20 to 45 was the largest in German history and constituted the highest relative percentage of the German population of any period before or since. In the year 1890 this age group constituted 34.4 per cent of Germany's population. In 1933 it peaked to make up 41.5 per cent of all Germans. By 1959 the 20 to 45 age cohorts had dropped to only 33.7 per cent of the German people. "From a demographic viewpoint," says Moller, "the economic depression hit Germany at the worst possible time: employment was shrinking precisely at a time when the employable population reached its postwar peak."⁴³

⁴² Carl Mierendorff, "Gesicht und Charakter der Nationalsozialistischen Bewegung," *Die Gesellschaft* (Berlin), 30 (1930): 497, 498, respectively.

⁴³ Herbert Moller, "Youth as a Force in the Modern World," *Comparative Studies in Society and History*, 10 (1968): 243, 244.

There is ample evidence that this generation of German youth was more inclined toward violent and aggressive, or what psychoanalysts call "acting-out," behavior than previous generations. At this point the explanations offered for this phenomenon are inadequate in their one-dimensionality. To say that the youth craved action or that they sought comfort in the immersion in a sheltering group is to beg the question of what made this generation of German youth different from all previous generations. What unique experiences did this group of people have in their developmental years that could induce regression to infantile attitudes in adulthood? One persuasive answer lies in fusing the knowledge we have of personality functioning from psychoanalysis—the most comprehensive and dynamic theory of personality available to the social and humanistic sciences today—with the cohort theory of generational change from historical demography and with the data on the leadership and structure of the Nazi party that we have from the researches of political scientists, historians, and sociologists.

IN THE HALF CENTURY prior to World War I Germany was transformed from an agricultural to an industrial economy, and her population grew from an agriculturally self-sufficient forty million to sixty-seven million by 1913. This mounting industrial population made her increasingly dependent on the importation of foreign foodstuffs. In the decade preceding World War I, five-sixths of Germany's vegetable fats, more than half of her dairy goods, and one-third of the eggs her people consumed were imported. This inability to be self-sufficient in foodstuffs made the German population particularly susceptible to the weapon of the blockade. The civilian population began to feel the pressure of severe shortages in 1916. The winter of 1916–17 is still known as the infamous "turnip winter," in which hunger and privation became widespread experiences in Germany. Getting something to eat was the foremost concern of most people. The official food rations for the summer of 1917 were 1,000 calories per day, whereas the health ministry estimated that 2,280 calories was a subsistence minimum. From 1914 to 1918 three-quarters of a million people died of starvation in Germany.⁴⁴

The armistice of November 11, 1918, did not bring the relief that the weary and hungry Germans anticipated. The ordeal of the previous three years was intensified into famine in the winter of 1918–19. The blockade was continued until the Germans turned over their merchant fleet to the Allies.⁴⁵ The armistice blockade was extended by the victorious Allies to include the Baltic Sea, thus cutting off trade with Scandinavia and the Baltic states.⁴⁶ Although the Allies undertook responsibility for the German food

⁴⁴ Karl Dietrich Erdmann, "Die Zeit der Weltkriege," in Bruno Gebhardt, ed., *Handbuch der Deutschen Geschichte* (Stuttgart, 1963), 4: 49, 77.

⁴⁵ James A. Huston, "The Allied Blockade of Germany 1918–1919," *Journal of Central European Affairs*, 10 (1950): 161.

⁴⁶ Erdmann, "Die Zeit der Weltkriege," 88.

supply under Article 26 of the Armistice Agreement,⁴⁷ the first food shipment was not unloaded in Hamburg until March 26, 1919.⁴⁸ On July 11, 1919, the Allied Supreme Economic Council decided to terminate the blockade of Germany as of the next day, July 12. Unrestricted trade between the United States and Germany was resumed three days later, on July 15.⁴⁹

The degree of German suffering under the postwar Allied blockade is a matter on which contemporary opinions differed. Some Allied diplomats and journalists charged that the German government exaggerated the plight of her people in order to increase Allied food deliveries.⁵⁰ Today the weight of the historical evidence is that there was widespread extreme hunger and malnutrition in the last three years of the war, which was intensified by the postwar blockade. We may concur with the evaluation of two American historians that "the suffering of the German children, women, and men, with the exception of farmers and rich hoarders, was greater under the continued blockade than prior to the Armistice."⁵¹

Among the documents that Mathias Erzberger, the chairman of the German Armistice Commission in 1918, requested from the Reichsgesundheitsamt (Reich's public health service) was a memorandum discussing the effects of the blockade on the civilian population. The memorandum, entitled "Damage to the Strength of the German People due to the Enemy Blockade Which Contravenes International Law," was submitted on December 16, 1918. This document is of special psychological interest because it consists of statistics giving increases in deaths, disease, stillbirths, and loss of strength in the labor force, all of which bear sums indicating monetary losses per individual and to the nation. The most remarkable set of figures are those that conclude that, on the basis of a population of 50 million with an average weight of 114.4 pounds, who have each lost one-fifth of their weight, the German people have lost 520,000 tons of human mass (*Menschenmasse*). The memorandum goes on to estimate that 1,560,000 to 1,768,000 tons of food would be necessary to restore the flesh (*Fleische*) that had been lost according to the previous calculation.⁵²

⁴⁷ *Der Waffenstillstand 1918-1919* (Berlin, 1928), 1: 49.

⁴⁸ Huston, "Allied Blockade of Germany," 162.

⁴⁹ United States Embassy, Paris, to War Trade Board, "Trade Resumption between the United States and Germany: Remaining Export Restrictions, telegram, July 15, 1919, in Suda Lorena Bane and Ralph Haswell Lutz, eds., *The Blockade of Germany after the Armistice 1918-1919: Selected Documents of the Supreme Economic Council, Superior Blockade Council, American Relief Administration and Other Wartime Organizations* (Stanford, 1942), 558-60.

⁵⁰ James A. Logan, Jr., memorandum to Herbert Hoover, Mar. 6, 1919, in *ibid.*, 184-88. See also "Food for Germany," *Daily News* (London and Manchester), Dec. 16, 1918; and John C. Van Den Veer, "The 'Hunger' Blockade: Truth about 'Starving Germany,'" *Sunday Times* (London), July 13, 1919, both quoted in *ibid.*, 670-71, 796-98 respectively.

⁵¹ Bane and Lutz, introd. to *ibid.*, v.

⁵² Dr. Rubner, "Notwendigkeit der Wiederauffütterung der durch die Blockade abgehungerten Bevölkerung," in Dr. Albrecht Philipp, MDR, et al., eds., *Das Werk des Untersuchungsausschusses der Verfassungsgebenden Deutschen Nationalversammlung und des Deutschen Reichstages 1919-1928, Reihe 4: Die Ursachen des Deutschen Zusammenbruches im Jahre 1918, Abteilung 2: "Der Zusammenbruch," Band 6* (Berlin, 1928): 419-42. The psychology of this

The demographic and statistical data constitute an overwhelming case that the German civil population, particularly infants and children, suffered widely and intensively during the war and blockade. Public health authorities and medical researchers have compiled population studies indicating damage to health, fertility, and emotions from 1914 to 1920. These are quantifiable indexes of physical deprivation from which the equally damaging but much more difficult-to-measure facts of emotional deprivation may be inferred.

On the grossest level the figures show a decline in the number of live births from 1,353,714 in 1915 to 926,813 in 1918. The birth rate per 1,000 population, including stillbirths, declined from 28.25 in 1913 to 14.73 in 1918. The number of deaths among the civilian population over one year old rose from 729,000 in 1914 to 1,084,000 in 1918. While there was a decline in deaths from causes related to nutrition and caloric intake, such as diabetes mellitus, alcoholism, obesity, diseases of the gastrointestinal tract, as well as a decrease in suicides, the gross mortality of the German population increased due to malnutrition, lack of heating, and consequent weakened resistance to disease. Specific causes of death that increased sharply during the war were influenza, lung infections and pneumonia, tuberculosis, diseases of the circulatory system, diphtheria, typhus, dysentery, and diseases of the urinary and reproductive organs.⁵³ All these diseases indicate a population whose biological ability to maintain health and to counter infection had been seriously undermined in the war years.

Upon looking at the comparative statistics for neonates and infants, we

quantification of human flesh is itself a subject requiring psychohistorical analysis. The high degree of isolation of feelings permitted by the use of statistics gives them an attractiveness to social scientists who wish to avoid their own painful emotions. "Statistics do not cry or bleed." This is not to question the undeniable advances in historical understanding that have been achieved by quantitative methods, but it is to suggest that these methods are ego syntonic for personalities who need emotional defenses against experience. It was by this mental process of compulsive depersonalizing and bureaucratic de-emotionalizing of experience that the suicidal depression of many Germans in 1918 was converted to the genocidal defense against depression by turning human beings into tons of hair and fertilizer a quarter of a century later. This was the active re-experiencing of a passively endured trauma of starvation and dehumanization. The depersonalization of personal suffering into statistical efficiency is also the complaint of many of the physicians who had the assignment of appraising claimants for restitution from the post-World War II German Federal government. Martin Wagh calls the demand for the precise measurement of misery of the survivors of concentration camps "an almost unanswerable question." "How," he asks, "is one to evaluate in percentages the distortions of personality growth, the warping of what should have been the 'normal' development of these children? Their ego functions have been profoundly affected by early object loss or object absence, the function of anticipation of danger has been severely altered, depressive moods are built into the character as stable structure and superego attitudes have often been monstrously deformed by archaic prototypes into over-religiosity or demands for absolute goodness. They cannot hate or, conversely, they fanatically endorse selfishness, litigiousness and belligerence. Indeed, does the question of M.d.E. [*Minderung der Erwerbsfähigkeit*], loss of earning capacity, make any sense at all? . . . The true loss of those who underwent such man-made disasters—and of those who are undergoing them today—is the very ability to experience happiness, and for this loss there is no measure." In Hoppe *et al.*, "Emotional Reactions of Psychiatrists when Confronting Survivors of Persecution," 200–01.

⁵³ Dr. Roesle, "Die Geburts und Sterblichkeitsverhältnisse," in Franz Bumm, ed., *Deutschlands Gesundheitsverhältnisse unter dem Einfluss des Weltkrieges* (Stuttgart, 1928), 1: 15, 17, 25, 58.

find a decline in weight and size at birth, a decline in the ability of mothers to nurse, a higher incidence of disease, particularly rickets and tuberculosis, as well as an increase in neurotic symptoms such as bed-wetting and an increment in the death rate. In the third year of the war the weight of neonates was 50 to 100 grams less at birth than before the war. In one Munich clinic in the year 1918 the females averaged 50 grams and the males 70 grams less at birth than in peacetime.⁵⁴

During the first year of the war more mothers nursed babies and the period of breast feeding was longer than previously, but by the winter of 1915 a decline in breast feeding had set in that was to continue through 1919. This is attributed to the war work of mothers and the "prolonged malnutrition and the damaged body of the mother due to psychic insult."⁵⁵ One chemical analysis done in Berlin found a marked decline in the quantity and quality of mother's milk resulting in the retarded development of breast-fed children and a delay in their normal weight gain. Infants fed on cow's milk also received milk that was short of nutriment, butterfat, and vitamins because of the lack of feed for the milk cows and the skimming off of cream for butter production.⁵⁶ To the shortage and inferior quality of milk must be added the almost total absence of fresh vegetables and fruit, important sources of vitamins, in the diets of children during the war and postwar period.

Not only infants but small children also were materially deprived by malnutrition. By the third year of the war children in the third year of life were up to 2.2 pounds lighter than normal body weight for their age. A study comparing 300 Berlin children in 1919 with figures from 1908-09 showed that the boys were retarded in growth to the level of children 1.5 years younger, and the girls were 1.25 years behind normal.⁵⁷

Like the infants, young children were also particularly afflicted with rickets, tuberculosis, and parasites. A medical examination of 2,154 children between 1914 and 1921 found that 39.1 per cent had rickets. Of the children in this group who fell ill between 12 and 18 months of age, 49.2 per cent had rickets. Cases of childhood miliary tuberculosis in the state of Baden rose 50 per cent after December 1918. A comparative sample of Berlin children aged three showed 8.1 per cent infected with tuberculosis in 1918; this rose to 29.9 per cent in 1919.⁵⁸

The pattern of increased illness and death among infants and small children in Germany carried through to children of school age. Deaths of children between 5 and 15 years of age more than doubled between 1913 and 1918. Using figures for 1913 as a base of 100, the death figures for this age

⁵⁴ L. Langstein and F. Rott, "Der Gesundheitsstand unter den Säuglingen und Kleinkindern," in *ibid.*, 90.

⁵⁵ *Ibid.*, 91.

⁵⁶ *Ibid.*, 92.

⁵⁷ *Ibid.*, 93, 95.

⁵⁸ *Ibid.*, 99, 100, 102.

group in 1918 were 189.2 for boys aged 5 to 10, and 215 for boys aged 10 to 15. Among the girls the death rates for these age groups were 207.3 and 239.9 respectively.⁵⁹

Among the leading causes of illness and death in this age group, as with the younger children, were rickets and tuberculosis. Corresponding losses in size and weight relative to age are also recorded. The medical statistics demonstrate an increased incidence among children of gastrointestinal disorders, worms, fleas, and lice. Psychological indications of stress among school children include an "enormous increase" in bed-wetting, "nervousness," and juvenile delinquency.⁶⁰

The evidence for deprivation is supported from Allied and neutral sources. The British war correspondent Henry W. Nevins reported from Cologne in March 1919 that tuberculosis had more than doubled among women and children and that the death rate among girls between 6 and 16 years had tripled. Because the children were so weak, school hours were reduced from seven to two hours daily. He wrote, "Although I have seen many horrible things in the world, I have seen nothing so pitiful as these rows of babies feverish from want of food, exhausted by privation to the point that their little limbs were like slender wands, their expression hopeless, and their faces full of pain."⁶¹

The British medical journal *Lancet* reported comparative figures derived from official German sources showing that the effect of food scarcity on the health of the German population was felt after mid-1916 but was stilled by skillful press censorship in wartime Germany. Among children from 1 to 5 years old the mortality was 50 per cent greater in 1917 than the norm of 1913. Among the children aged 5 to 15 mortality had risen 75 per cent.⁶²

A tripartite commission of doctors was appointed by the medical faculties of the Netherlands, Sweden, and Norway to examine health conditions in Germany after the cessation of hostilities. This neutral medical commission found a state so deplorable that John Maynard Keynes was moved in 1920 to ask with prescience: "Who can say how much is endurable, or in what direction men will seek at last to escape from their misfortunes?"⁶³ The physicians reported on the effects of prolonged hunger and malnutrition.

Tuberculosis, especially in children, is increasing in an appalling way, and, generally speaking, is malignant. In the same way rickets is more serious and widely prevalent. It is impossible to do anything for these diseases; there is no milk for the tuberculosis, and no cod-liver oil for those suffering from rickets. . . . Tuberculosis is assuming almost unprecedented aspects, such as have hitherto only been

⁵⁹ Dr. Stephani, "Der Gesundheitsstand unter den Schulkindern," in *ibid.*, 117.

⁶⁰ *Ibid.*, 129, 122-23.

⁶¹ Henry W. Nevins, "Babies 'Withering Away,'" *Daily News* (London and Manchester), Mar. 13, 1919; and his "Famine in Europe," *Nation* (New York), Mar. 8, 1919, both quoted in Bane and Lutz, *Blockade of Germany*, 731, 727, respectively. See also Nevins's report carried as "Starving Europe" in *Herald* (London), Jan. 18, 1919, also quoted in *ibid.*, 701.

⁶² "The European Food Situation," *Lancet* (London), Mar. 8, 1919, quoted in *ibid.*, 726-27.

⁶³ John Maynard Keynes, *The Economic Consequences of the Peace* (New York, 1920), 251.



Berlin soup kitchen, 1916. Photograph from Annemarie Lange,
Das wilhelminische Berlin (East Berlin, 1967).

known in exceptional cases. The whole body is attacked simultaneously, and the illness in this form is practically incurable. . . . Tuberculosis is nearly always fatal now among adults. It is the cause of 90 per cent of the hospital cases. Nothing can be done against it owing to lack of foodstuffs. . . . It appears in the most terrible forms, such as glandular tuberculosis, which turns into purulent dissolution.⁶⁴

Contemporary German sources confirm this report. A writer for a prestigious liberal newspaper accompanied the Hoover Commission to the Erzgebirge where there was severe famine. He wrote:

I visited large country districts where 90 per cent of all the children were rickety and where children of three years are only beginning to walk. . . . Accompany me to a school in the Erzgebirge. You think it is a kindergarten for the little ones. No, these are children of seven and eight years. Tiny faces, with large dull eyes, over-

⁶⁴ Swedish press of Apr. 1919, as reported in *ibid.*, 250n.

shadowed by huge puffed, rickety foreheads, their small arms just skin and bone, and above the crooked legs with their dislocated joints the swollen, pointed stomachs of the hunger oedema.⁶⁵

World War I was the first total war in history—it involved the labor and the commitment of full energies of its participant peoples as no previous war had. The men were in the armed services, but a modern war requires a major industrial plant and increased production of foodstuffs and supplies to support the armies. Yet the number of men working in industry in Germany dropped 24 per cent between 1913 and 1917. In the state of Prussia in 1917 the number of men working in plants employing over ten workers was 2,558,000, including foreigners and prisoners of war, while in 1913 the total of men employed had been 3,387,000.⁶⁶

In Germany this meant a shift of major proportions of women from the home and domestic occupations to war work. In the state of Prussia alone the number of women engaged in industrial labor rose by 76 per cent, from 788,100 in 1913 to 1,393,000 in 1917. For Germany as a whole 1.2 million women newly joined the labor force in medium- and large-sized plants during the war. The number of women workers in the armaments industry rose from 113,750 in 1913 to 702,100 in 1917, a gain of 500 per cent. The number of women laborers who were covered under compulsory insurance laws on October 1, 1917, was 6,750,000. The increase of adult female workers in Prussia in 1917 was 80.4 per cent over 1913. The number of women railroad workers in Prussia rose from 10,000 in 1914 to 100,000 in 1918, an increase of 1,000 per cent.⁶⁷

Another new factor in the labor force was the youthful workers. The number of adolescents aged 14 to 16 employed in chemical manufacturing increased 225 per cent between 1913 and 1917. For heavy industry the corresponding figure was 97 per cent. Many of these were young girls aged 16 to 21. This age group constituted 29 per cent of all working women.⁶⁸

That German women were massively engaged in war work was recognized as having resulted in the neglect of Germany's war children and damage to the health of the mothers.⁶⁹ Reports came from government offices of increased injuries to children of ages 1 to 5 years due to lack of supervision.⁷⁰ S. Rudolf Steinmetz evaluates the demoralization of youth between 1914 and 1918 as an indirect consequence of the war. He ascribes to "the absence of many fathers, the war work of many mothers" the damaged morals and morality of youth.⁷¹

⁶⁵ *Fossische Zeitung*, June 5, 1919, as reported and translated in *ibid.*, 250n.–51n.

⁶⁶ Marie-Elisabeth Lüders, *Das Unbekannte Heer: Frauen Kämpfen für Deutschland, 1914–1918* (Berlin, 1937), 85, 85 n.1.

⁶⁷ *Ibid.*, 84, 85, 86, 151, 151 n.1, 153 n.2.

⁶⁸ *Ibid.*, 85, 86.

⁶⁹ Zunehmende Vernachlässigung der Kinder sowie wachsende gesundheitliche und sittliche Gefährdung der Arbeiterinnen waren unverkennbar. *Ibid.*, 91.

⁷⁰ *Ibid.*, 128 n.1.

⁷¹ S. Rudolf Steinmetz, *Soziologie des Krieges* (Leipzig, 1929), 169.

Many of the war-related phenomena under discussion were not unique to the Central European countries. The factor of a chauvinistic atmosphere of war propaganda was certainly present in all belligerent countries. The absence of the parents in wartime service was also not unique to Germany or Austria. The children of other countries involved in the war too had absent parents and were often orphaned. French and British families undoubtedly experienced the sense of fatherlessness and desertion by the mother as much as did German and Austrian families. Two added factors, however, make the critical difference in the constellation of the child's view of the world: the absence of German and Austrian parents was coupled with extreme and persistent hunger bordering in the cities on starvation, and when the German or Austrian father returned he came in defeat and was unable to protect his family in the postwar period of unemployment and inflation. Not only was the nation defeated, but the whole political-social world was overturned. The Kaiser of Germany had fled, and the Kaiser of Austria had been deposed. Some Germans would say that the Kaiser had deserted his people, to be replaced by an insecure and highly ambivalent republic under equivocating socialist leadership. Much more than an army collapsed—an entire orientation to the state and the conduct of civic life was under assault in 1918–19. These national factors unique to Central Europe exacerbated the familial crisis of the absence of parents and made of this wartime experience a generational crisis.

TODAY IT IS WIDELY RECOGNIZED that the emotional constellation of the childhood years is decisive for the future psychological health and normality of the adult. Modern war conditions, through the long-term breakup of family life, added in some cases to a lack of essential food and shelter, and a national atmosphere highly charged with unmitigated expressions of patriotism, hatred, and violence must inevitably distort the emotional and mental development of children, for imbalance in the fulfillment of essential psychic and bodily needs in childhood results in lasting psychological malformations.

It may be helpful to review briefly modern theories of phase-specific development and emotional growth from infancy to adulthood in order to point to the areas of greatest potential stress due to family or social trauma. What follows is necessarily no more than a theoretical model of development, an ideal typology of the psychodynamics of personality development that will be useful as a heuristic device against which to test empirical and cultural data. It does not presume to be a precise model of any single individual's development.

More is now known than ever before about the psychological processes and fantasies of children. There is a high level of agreement among child-guidance specialists that maternal deprivation of the child has long-ranging effects on the mental health and emotional strength of the adult. The first

relationship a child forms is with his mother.⁷² His attitude to the object—in the first case, the mother—is a passive, receptive one; that is, the child is narcissistic and selfish, he wishes to be given pleasure and to have his discomforts removed. A number of British psychoanalysts of what has come to be known as the “English school” have stressed the quality of destructive oral rage that is normally present in all children. This cataclysmic world-destroying rage is, of course, intensified in cases of deprivation.

The late British pediatrician and psychoanalyst D. W. Winnicott described the rages experienced by infants in which they want to destroy their mother’s breasts and believe they can do so by feeding from them.⁷³ Melanie Klein also pictures the world of the infant as a seething cauldron of intense emotions of love and hate in which the baby is dominated by impulses to destroy the very object of all his desires—his mother.⁷⁴ When describing the baby’s uncontrollable sensations of rage and his experience of threatened destruction from within and without, Joan Riviere writes:

He becomes aggressive. He automatically explodes, as it were, with hate and aggressive craving. If he feels emptiness and loneliness, an automatic reaction sets in, which may soon become uncontrollable and overwhelming, an aggressive rage which brings pain and explosive, burning, suffocating, choking bodily sensations; and these in turn cause further feelings of lack, pain and apprehension. The baby cannot distinguish between “me” and “not me”; his own sensations are his world, *the* world to him; so when he is cold, hungry or lonely there is no milk, no well-being or pleasure in the world—the valuable things in life have vanished. And when he is tortured with desire or anger, with uncontrollable, suffocating screaming, and painful, burning evacuations, the whole of his world is one of suffering; it is scalded, torn and racked too.⁷⁵

René Spitz, in his classic studies of hospitalism, has shown that the absence of an emotionally available mother during the child’s first year damages his physical development as well as his personality. Spitz compared children who were cared for by their own mothers in a prison nursery with children in an orphanage whose care was in the hands of professionally competent nurses but who had no close personal care or contact with their mothers. Although on admission the children in the orphan home rated much higher in body-mastery, development, and achievement indexes, within four months they deteriorated and continued to sink. They were unable to speak, feed themselves, or to acquire habits of cleanliness. The infants in the prison nursery went through a progressive development because they had an intense emotional interchange with their mothers during the first twelve months of life.⁷⁶

⁷² I use the masculine pronouns for convenience. The infantile object relationships to the mother are not sexually differentiated; they are the same for the girl or the boy.

⁷³ D. W. Winnicott, “Aggression” (1939), in his *The Child and the Outside World: Studies in Developing Relationships*, ed. Janet Hardenberg (New York, 1957), 170.

⁷⁴ Melanie Klein, “Love, Guilt and Reparation,” in *Love, Hate and Reparation* (New York, 1964), 58.

⁷⁵ Joan Riviere, “Hate, Greed and Aggression,” in *ibid.*, 8–9.

⁷⁶ René A. Spitz, “Hospitalism: An Inquiry into the Genesis of Psychiatric Conditions in Early Childhood,” in *The Psychoanalytic Study of the Child*, 1 (1945): 53–74; and his “Hospitalism: A Follow-up Report on Investigation Described in Volume I, 1945,” in *ibid.*, 2 (1946): 113–17.

A somewhat later and very great threat to a child's security is the trauma of separation. It is considered to be essential for sound personality development that the infant and young child should experience a warm, intimate, and continuous relationship with a mother or permanent mother figure, a relationship in which both infant and mother find gratification and pleasure. John Bowlby observes: "Prolonged breaks [in the mother-child relationship] during the first three years of life leave a characteristic impression on the child's personality. Clinically such children appear emotionally withdrawn and isolated. They fail to develop libidinal ties with other children or with adults and consequently have no friendships worth the name."⁷⁷

For the infant and child the mother is the supreme agent who can give gratification and assuage pain. "The absence of the mother," writes Margaret Mahler, "exposes the normal infant . . . to the danger of helplessness and longing, with consequent anxiety."⁷⁸ The danger is particularly threatening to the child not only because of his utter dependence and helplessness but because of his own acute ambivalence. There is a great accumulation of aggression toward love objects during the oral-sadistic, anal-sadistic, and Oedipal phases of child development. The child must struggle with intense fears of loss of love due to his own hostility and aggression. He must preserve his love for the object (mother) despite his rage and fear. If the mother's love and acceptance of the child is not forthcoming, he reacts as if he has been rejected for his badness. There is a deficit in self-esteem.⁷⁹ The child views himself as unlovable and worthless, as an evil creature who drives loved ones away. His healthy narcissistic balance is destroyed, and his ego is weakened. One way of coping with feelings of inner badness is to project these evil, asocial parts of the self out onto others.

Bowlby terms separation from the mother or mothering figure the "primal anxiety" in the life of a young child. The condition of separation causes intense alarm, fright, and distress. Because of the mother's tremendous importance for the child's survival, the response of separation anxiety is permanently ready for activation; it is easily activated and cannot be completely terminated except by the child's preferred mother figure.⁸⁰

Some specialists in the problems of childhood separation and individuation suggest that the desire to merge with a mass movement in adolescence and adulthood expresses the need to regress, while in a state of panic or terror, to the preindividuation phase. The merging may be a crowd fusing with each other or with an authoritarian regime and its dictatorial leader.⁸¹

Separation from the mother engenders hostility because it is interpreted as rejection by the loved object; it is experienced as the loss of love. The

⁷⁷ John Bowlby *et al.*, *Maternal Care and Mental Health and Deprivation of Maternal Care* (New York, 1966), 11, 32.

⁷⁸ Margaret S. Mahler, *On Human Symbiosis and the Vicissitudes of Individuation* (New York, 1968), 1: 234.

⁷⁹ *Ibid.*, 222.

⁸⁰ John Bowlby, "Separation Anxiety," *International Journal of Psychoanalysis*, 41 (1960): 105.

⁸¹ Edith Jacobson, *The Self and the Object World* (New York, 1964), 41 n.4.

period when this anxiety and hostility is most active is also the period when patterns of control and of regulating conflict are laid down. Thus, separation anxiety and hostility are provoked by the same experience. The hostility must be repressed because it is directed at the loved object and to express it and risk further loss is far too dangerous. Being repressed, the hostility generates further anxiety.⁸² Both the increased need for the mother and the heightened unconscious hostility toward her promote a neurotic, anxiety-prone personality inclined to regress to primal anxiety and rage when confronted with frustration in later life.⁸³

Children are traumatized by the horrors of war, by hearing reports and seeing actual pictures of killed and maimed fathers, mothers, and dead children. But it is a fantasy of the innocence of childhood and a misconception of the nature of children to believe that destruction and aggression are unknown to them.

Aggression, of course, does not end in infancy and childhood. What Winnicott, Riviere, Klein, Spitz, Bowlby, and Mahler describe as anger and rage in neonates and infants is observable as destructive behavior in any nursery as the infant becomes a child. Anna Freud and Dorothy T. Burlingham describe their observations of the conflicts with aggression and control that characterize the struggle of bowel training:

Children between the ages of one and two years, when put together in a playpen will bite each other, pull each other's hair and steal each other's toys without regard for the other child's unhappiness. They are passing through a stage of development where destruction and aggression play one of the leading parts. If we observe young children at play, we notice that they will destroy their toys, pull off the arms and legs of their dolls or soldiers, puncture their balls, smash whatever is breakable. . . . The more their strength and independence are growing the more they will have to be watched so as not to create too much damage, not to hurt each other or those weaker than themselves.

The authors then add a highly significant sentence: "We often say, half jokingly, that there is a continual war raging in a nursery."⁸⁴

The young child experiences murderous death wishes toward all people who have disturbed, offended, or rejected him in fantasy or reality. The

⁸² Bowlby, "Separation Anxiety," 108-09.

⁸³ The critical importance of early childhood separation from the mother is demonstrable in the clinical setting of psychoanalysis, particularly in the transference relationship to the psychoanalyst. Separation anxiety is characteristically exacerbated on the eve of holidays and other interruptions in the analysis. In some cases separation phenomena will be expressed by depressions on the weekend when the analytic routine is interrupted and retaliatory desires to leave first of an "I am going to leave you before you can leave me" variety. Patients under the dominance of separation anxiety react to the interpretations of the analyst as though they are being fed. They are insatiable, demanding, and they drink in every word indicating that they are reacting on an oral level. See Ralph G. Greenson, *The Technique and Practice of Psychoanalysis* (New York, 1967), 1: 240. These patients sometimes somatize their anxiety by getting sick during the analyst's absence, or will try to invade his private life. Their dreams often include disasters or accidents occurring to the loved person, demonstrating the unconscious hostile ambivalence that exists. Fusion with the analyst serves as reassurance against hostile death wishes, as it is evident that no harm has occurred if the loved one is present and therefore protected from the patient's hostility.

⁸⁴ Anna Freud and Dorothy T. Burlingham, *War and Children* (New York, 1943), 21-22.

jealous desire to do away with an interfering sibling or rival is a universal commonplace. One of the most important social aims of education is to curb the unmitigated aggressiveness of children. At first direct action on destructive wishes is prohibited by outside authority. Later the child learns to inhibit these impulses in himself. They are defended against by reaction formations such as compassion and pity, and compulsive defenses such as scrupulous care and meticulousness. They may be repressed or sublimated into competitive and constructive activity. The child learns to criticize and overcome in himself his hostile, antisocial wishes, which is to say that he refuses them conscious expression. He accepts that it is bad to hurt, cripple, and kill. He believes that he has no further wish to do any of these violent and destructive things. He can only maintain this belief, however, if the outer social world is supportive of his struggle by likewise curbing its aggression.

When a child who is struggling with his aggressive and destructive impulses finds himself in a society at war, the hatred and violence around him in the outer world meet the as yet untamed aggression raging in his inner world. At the very age when education is beginning to deal with the impulses in the inner environment the same wishes receive sanction and validation from a society at war. It is impossible to repress murderous and destructive wishes when fantasied and actual fighting, maiming, and killing are the preoccupation of all the people among whom the child lives. Instead of turning away from the horrors and atrocities of war, he turns toward them with primitive excitement. The very murderous and destructive impulses that he has been trying to bury in himself are now nourished by the official ideology and mass media of a country at war.

The power of his aroused inner fantasies of violence is anxiety-producing for the child. It is as though an inner signal alerts him to beware of the danger of losing control. When, in addition, the child is not with his family, he will often develop the symptoms of nervousness, bed-wetting, fecal incontinence, stealing, truancy, and delinquency that Winnicott describes.⁸⁵

Many political scientists and historians have pointed to the function of National Socialism as a defense against emotional insecurity. Harold Lasswell, in contrast to those who have interpreted Hitler as a father or a son symbol,⁸⁶ develops precisely the theme of Hitler's maternal function for the German people, suggesting that nazism was a regressive attempt to compensate for mothering and family life that had been inadequate. Lasswell stresses the imagery of cleanliness and pollution of the anal phase.

There is a profound sense in which Hitler himself plays a maternal role for certain classes in German society. His incessant moralizing is that of the anxious

⁸⁵ Winnicott, "Residential Management as Treatment for Difficult Children" (1947), in *The Child and the Outside World*, 100.

⁸⁶ These interpretations are not necessarily mutually exclusive. Hitler, in accord with the principles of multiple function and overdetermination, may have represented mothering, fathering, and filial roles to the same people at various times and to different groups of people at the same time.

mother who is totally preoccupied with the physical, intellectual and ethical development of her children. He discourses in public, as he has written in his autobiography, on all manner of pedagogical problems, from the best form of history teaching to the ways of reducing the ravages of social disease. His constant preoccupation with "purity" is consistent with these interests; he alludes constantly to the "purity of the racial stock" and often to the code of personal abstinence or moderation. This master of modern Galahadism uses the language of Protestant puritanism and of Catholic reverence for the institution of family life. The conscience for which he stands is full of obsessional doubts, repetitive affirmations, resounding negations and stern compulsions. It is essentially the bundle of "don'ts" of the nursemaid conscience.⁸⁷

Similarly, research indicates that paternal deprivation in childhood, which assumes increasing importance in later years as the child approaches and works through his Oedipal conflict,⁸⁸ also has a profound impact on the personality and ideas of youth concerning father images, political authority, and sources of power.⁸⁹ In a study comparing father-separated from father-at-home elementary school children, George R. Bach found that "father separated children produce an idealistic fantasy picture of the father" that "seem[s] to indicate the existence of strong drives for paternal affection." In turn, then, "the severely deprived [*sic*] drive for paternal affection provides strong instigation for the idealistic, wish-fulfilling fantasies."⁹⁰ The absent father is idealized. This is in part a reaction formation—that is, a de-

⁸⁷ Harold D. Lasswell, "The Psychology of Hitlerism as a Response of the Lower Middle Classes to Continuing Insecurity" (1933), reprinted in his *The Analysis of Political Behavior: An Empirical Approach* (Hamden, Conn., 1966), 240–41.

⁸⁸ During the period of the Oedipal conflict, roughly from ages three to six, the child has feelings of jealousy and hatred toward his father and his siblings combined with the desire to be loved by his mother. This conflict also gives rise to fear of his rival lest the father retaliate by mutilating and castrating him because of his intense hostility to his father. The Oedipal conflict ends in the boy's repressing his passionate love for his mother and replacing it by tender feelings toward her and by repressing his mixed feelings of love and hate for his father, and also replacing them with tender feelings toward him. The child begins to seek object relationships outside of the family in teachers, in relatives, and in friends of the same age and sex.

⁸⁹ Lois Meek Stolz *et al.*, *Father Relations of War-Born Children* (Stanford, 1954), 192–207. Weinstein and Platt argue that after the mid-nineteenth century the Central European father had lost his earlier household and nurturant functions in the family. He became the social representative to his sons of the standard of a highly competitive economic and social order that condemned passive and dependent gratifications. Industriousness and calculation were the lessons that he taught, and self-control and discipline were the restraints that he imposed as a consequence of his new role as a power outside of the home after changes imposed by the industrial and economic revolutions. *The Wish to be Free*, 148–52, 177–82.

⁹⁰ George R. Bach, "Father-Fantasies and Father-Typing in Father-Separated Children," *Child Development*, 17 (1946): 71. Bach's research showed that "beyond influencing the child through father-typing, the mother may actually modify the child's personality development in the direction of femininity during the period of father-absence. The father is not available for imitation or identification with masculine social behavior, and there is now more opportunity to imitate feminine attitudes, manners, and values of the mother. The idealistic father-fantasies of both the separated boys and the separated girls with their stereotyped, affectionate and non-aggressive themes are very similar to the doll play fantasies *characteristically produced by girls* (in contrast to boys) under ordinary family conditions. This "feminization" of the father-separated child's fantasy may then be a reflection of the increased potency of the mother as a social stimulus. The idealistic father-fantasies may, therefore, not only be an expression of the child's wish for an affectionate father but may actually also be symptomatic of a personality reorganization produced by exclusive maternal domination." *Ibid.*, 77.

fense against hatred toward the father by replacing these repressed hostile feelings with their conscious opposite.

Psychoanalytic theory and clinical evidence tell us that prolonged absence of the father results in intensified closeness to the mother. This in turn will heighten Oedipal conflict for the son in latency.⁹¹ Stimulated incestuous fantasies will increase the fear of punishment for the forbidden longings. The sharpened castration anxiety of the boy left alone with his mother results in strengthened identification with the absent idealized father and in homosexual longings for him. The homosexual feelings for the distant father are a love for him shared with the mother and a defense against heightened incestuous feelings for her.

The emancipation of women, which was accelerated greatly in World War I by the needs of a total war economy, gave to women what had been traditionally men's vocational roles and familial responsibilities. In such circumstances, in her own eyes and in the eyes of her children, the woman who works in industry and agriculture is now doing "man's" work. Thus the mother who manages the affairs of the family may acquire a "phallic" or masculine image to her children. As she is not accustomed to bearing the full responsibility for the family welfare and discipline, she might tend to become anxious. This anxiety is further exacerbated by her sexual and emotional frustration and concern for her husband. Anxieties of all kinds are immediately and inevitably communicated to children, who then become anxious as well. In her uncertainty a mother will often be more punitive than she would be under normal circumstances, both to ward off her own sexual feelings and because of anxiety about her role as disciplinarian. This heightens the passive masochism and castration anxiety in young boys.

Boys who become homosexuals are often those who were left alone with their mothers and formed an intense attachment to them that was unmediated by the father's presence and protection. The struggle against feminine identification and the regression to narcissistic object choice—that is, choosing someone who is like himself, what he was, or what he would like to be—are all greatly intensified in boys raised without fathers.⁹²

If early separation and deprivation damages the frustration tolerance and reality-testing functions of children, we must look at the process of the political socialization and political-fantasy formation of normal children. Research in the field of children's concepts of politics, political leadership, and national identity indicates that many of the primary identifications of a lifetime are already formed by the second grade of elementary school, that is at age eight

⁹¹ During the latency period—approximately ages six to eleven—there is a marked increase in the strength of the defenses against the sexual drives. This is the period when children are emotionally relatively calm, learn well, master their bodies, and develop their intelligence. The child acquires the equipment with which to encounter the onslaught of sexual-drive energy at puberty.

⁹² I am indebted to Oscar Sachs for his discussion in a personal communication of the homosexual dynamics of the Nazi generation.

or nine.⁹³ Children in elementary school develop predispositions for a political party, intense nationalistic chauvinism of a “we are good, they are bad” variety, and positive affectual attachment to symbols of patriotism such as the flag or the Statue of Liberty. “Affect,” David O. Sears points out, “precedes information. Children express strong positive affect toward leaders, and only later acquire supporting rationalizations.”⁹⁴ Familiarity with high leaders is practically at adult levels by the second grade. In Fred Greenstein’s sample, 96 per cent of American children aged nine knew who the president was.⁹⁵ In Robert D. Hess’s study 95 per cent of the children aged seven through nine recognized and correctly identified the president. A similarly high level of recognition was found for the national leaders in studies done in Chile, Japan, and Australia.⁹⁶

Children tend to idealize the president and to personalize the government—that is, they see it in terms of the person of the leader rather than as an institution in which people play roles. The extent to which children exaggerate the personal power and charisma of the leader is impressive. He has God-like qualities in the child’s imagery. Eighty-six per cent of second graders see the president of the United States as “running the country”;⁹⁷ 76 per cent of second graders think that the president makes the laws.⁹⁸ The president is viewed by children as benevolent and protective, powerful and strong.⁹⁹ In a study of 366 children in Chicago, 60 per cent of the second graders felt that the president is “the best person in the world.”¹⁰⁰

The mentality of a state of war complements the child’s most archaic psychic mechanisms for coping with himself and the world, the devices of splitting and projection. Splitting is what a people at war does by dividing the world into “good” and “bad” countries, those on our side who have only virtues and whom we love, and the enemy who is evil and whom we hate. We are thus enabled to get pleasure by gratifying our aggressive feelings. For the

⁹³ Marvin Rintala, a historian, explicitly argues for the years of “late adolescence and early adulthood” as “the formative years during which a distinctive personal outlook on politics emerges, which remains essentially unchanged through old age. The crucial years are regarded as approximately 17 to 25. If these years are in fact formative, neither the years preceding nor the years following them are decisive in the formation of political attitudes.” “Political Generations,” *International Encyclopedia of the Social Sciences*, 6: 93. This is undoubtedly an overstatement by Rintala. The earlier determinants of the political-socialization process, including the preschool years of infancy and childhood when the identifications that constitute the basic components of identity are formed, may not be discounted or ignored.

⁹⁴ David O. Sears, “Political Behavior,” *Handbook of Social Psychology* (2d ed.; Reading, Mass., 1969), 5: 415, 416.

⁹⁵ Fred I. Greenstein, *Children and Politics* (New Haven, 1965), 32.

⁹⁶ Robert D. Hess, “The Socialization of Attitudes toward Political Authority: Some Cross-National Comparisons,” *International Social Science Journal*, 25 (1963): 555.

⁹⁷ Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children* (Chicago, 1967), 35.

⁹⁸ David Easton and Jack Dennis, “The Child’s Image of the Government,” *Annals of the American Academy of Political and Social Sciences*, 361 (1965): 48.

⁹⁹ Greenstein, *Children and Politics*, 37–42; Greenstein, “The Benevolent Leader: Children’s Images of Political Authority,” *American Political Science Review*, 54 (1960): 934–43.

¹⁰⁰ Robert D. Hess and David Easton, “The Child’s Image of the President,” *Public Opinion Quarterly*, 24 (1960): 648–54.

child, too, there are two kinds of men, one "good" and one "bad." In wartime the absent father-soldier is idealized. He is glorified and any hostile feelings toward him are projected onto the evil enemy on the other side.¹⁰¹

Much of recent emphasis in psychoanalytic research and clinical work, particularly in psychoanalytic ego psychology, has been on the importance of the years of adolescence for character formation and identity resolution.¹⁰² These are the years when the basic choices and commitments of a lifetime are made after much painful searching, testing, and doubt.¹⁰³ What then happens when children who have been deprived become politically effective? How do they respond as adolescents to the frustrations of reality? There are many theoretical and empirical approaches to adolescent aggression. Albert Bandura and Richard H. Walters offer what is essentially a social-modeling or imitational view of adolescent aggression. For us their study is significant because it shows that aggressive boys come from families where they have experienced deprivation of affectional nurturance.¹⁰⁴ The post-Oedipal child has to repress his sexual and hostile impulses in favor of affectionate attachments to his parents. In adolescence the biological maturation process leads to a temporary revival of the Oedipal strivings. But now the incestuous sexual and hostile wishes must be finally relinquished. The adolescent's affectionate ties to his parents must also be sufficiently loosened to guarantee his future freedom of object choice and a sound adjustment to social reality. His practical and emotional dependency on his parents must be definitely and finally abandoned. This detachment from parental authority is, said Sigmund Freud,

¹⁰¹ George L. Mosse specifies the function of anti-Semitism as a displacement for the frustrations of the postwar children. He asserts the role of anti-Semitic agitation in the Weimar Republic was to provide "the children with a clearly defined object to vent their frustrations on, an identifiable obstacle to their aspirations which could be blamed for all their failures in later life." *The Crisis of German Ideology: Intellectual Origins of the Third Reich* (New York, 1964), 267.

¹⁰² Adolescence is marked by a rapid increase in the rate of growth, by a resurgence of the erotic drives that were repressed after the Oedipal crisis, by allegiance to heroic ideals, and by the use of group ideals by the young men and women emerging from childhood. Adolescence is still far from adulthood. The period after puberty is one of the most volatile and unstable that the growing, not yet mature, person undergoes. His personality is still incomplete, requiring a reconstitution and remodeling of the psychic structures under the onslaught of renewed instinctual and emotional conflicts. The adolescent passes through violent, affective crises. He has rapid, sudden swings of mood. He tends to suffer from recurring painful states of depression and despair that may involve severe guilt conflicts, harassing feelings of shame and self-consciousness, and hypochondrial body preoccupations. In climbing the tortuous path to adulthood the adolescent experiences at every new step anxiety, confusion, disorganization, and a return to infantile positions, followed by reorganization and advance to more mature levels. He must not only free himself from his attachments to persons who were all-important in this childhood, he must also renounce his former pleasures and pursuits more rapidly than at any former stage of development.

¹⁰³ For the best discussions of psychological problems of adolescence see Erik H. Erikson, *Childhood and Society* (New York, 1963); and his *Identity and the Life Cycle* (New York, 1959); Peter Blos, *On Adolescence: A Psychoanalytic Interpretation* (New York, 1962); Gerald H. J. Pearson, *Adolescence and the Conflict of Generations* (New York, 1958); Anna Freud, *The Ego and the Mechanisms of Defence*, tr. Cecil Baines (New York, 1946), 149-93; and Jacobson, *Self and the Object World*, 159-216.

¹⁰⁴ Albert Bandura and Richard H. Walters, *Adolescent Aggression: A Study of the Influence of Child-Training Practices and Family Inter-relationship* (New York, 1959).

“one of the most significant, but also one of the most painful, psychical achievements of the pubertal period.”¹⁰⁵

In discussing the effects of childhood deprivation we have followed the phase-specific psychosexual development of the child. We saw, in order, the traumata of the oral phase, of separation-individuation from the mother, the struggles with aggression and control that constitute the anal phase, the Oedipal conflict, the latency years of grade-school political socialization, to the crisis of adolescence that precedes adulthood. Each phase has its special stresses and focuses of conflict. Each may become a point of fixation to be returned to at a later date if the turmoil has been too great or the storm too violent to permit the child passage unharmed.

WE MUST SEEK THE WIDEST possible type and range of clinical material, cultural documentation, and quantitative statistical data in our quest for historical evidence. This essay will present three bodies of historical materials, some from each of these categories of data: comparative, qualitative, and quantitative. All varieties of historical evidence have an important and complementary function in generating new hypotheses, contributing new insight, and demarking future areas for exploration.

Psychoanalytical interest was directed at the war generation almost contemporaneously with the events. As early as 1919 Paul Federn interpreted the psychological dimensions of the postwar strikes and the soldiers' and workers' councils that sprang up throughout Central Europe.¹⁰⁶ He viewed the loss of the national father figure, the Kaiser, who could no longer satisfy infantile fantasies of a father who is omnipotently powerful, wise, and strong, who offers absolute security and protection, as the traumatic psychological event of the war. Now the Kaisers of Germany and Austria were deprived of land, throne, power, and the ability to offer a feeling of security. Thus a fatherless society was created that no longer stood in awe of the state. For some sons of the state, Federn suggested, the disappointment came during the war when their leaders and army officers made irresponsible and sometimes impossible demands that condemned them to death. The soldiers' and workers' councils were seen as an attempt to establish a nonpatriarchal social order, a brotherhood to replace the defeated father. Such a situation is unstable, however. Federn in March 1919—the date is worth noting for it was the zenith of republicanism in Europe—predicts the demise of the republic in

¹⁰⁵ Sigmund Freud, “Three Essays on the Theory of Sexuality” (1905), *Standard Edition*, 7: 227. Erik H. Erikson emphasizes the marked adolescent quality of Hitler's self-created image before the German public. He interprets *Mein Kampf* as a skillful portrayal of a fantasy that would appeal to the psychic needs of many Germans of the postwar generation. It is the fantasy of the adolescent who never gave in and identified with the domineering father. He stubbornly never surrendered. Hitler presented himself as a glorified older brother, “an unbroken adolescent,” “a gang leader who kept the boys together by demanding their admiration, by creating terror, and by shrewdly involving them in crimes from which there was no way back. And he was a ruthless exploiter of parental failures.” *Childhood and Society*, 337.

¹⁰⁶ Paul Federn, *Psychologie der Revolution—Die vaterlose Gesellschaft* (Vienna, 1919).

Central Europe and a turn to dictatorship on the psychological grounds of prevailing family patterns and man's desire to be dominated. The fatherless society will not succeed. "Among those who have now freed themselves of the social father-son relationship, the tendency toward it still remains so strong, that they only wait for a suitable newly appearing personality who embodies their father ideal, in order to again relate as a son to him."¹⁰⁷

A study such as the present one, which attempts to assess the impact on children of a catastrophe like a war, should use the best clinical observations in comparative historical situations when these are available. If wartime deprivation has profound emotional effects on young children, these effects should not be limited to one time and place in the modern world.¹⁰⁸ The findings in Germany should also be evident in another industrial land and for other twentieth-century wars, such as for England in World War II.

The British experience is especially valuable to the historian who would consider the emotional effect of war on children because many English children were evacuated from their homes and families in London and the other big cities during World War II, and they were helped through this trying experience by the expert guidance of such specialists in the psychology of children as Anna Freud, Dorothy T. Burlingham, and D. W. Winnicott. These psychoanalysts carried out close residential observation of the evacuated children and published detailed studies of the children's responses and adaptations to the breakup of families in wartime. These were "normal" children, they were not hospitalized, nor were they juvenile offenders. They were not so heavily traumatized by their experience that their regressive defenses resisted all modification, as is the case with most of the children who survived concentration camps.¹⁰⁹ The blitzed English children were provided with a homelike environment and encouraged in every way toward normal development.¹¹⁰ The fact that they were out of their homes and away from their families provides a degree of objectivity to the observations. The data were not filtered through reports of the parents; they are first-hand observations by trained professionals.

Anna Freud and Burlingham found that while a child will accept mother substitutes in the absence of its own mother, "there is . . . no father substitute who can fill the place which is left empty by the child's own father." "The infant's emotional relationship to its father begins later in life than that of

¹⁰⁷ *Ibid.*, 28.

¹⁰⁸ For the case of an American pubescent boy suffering heightened aggressiveness and confusion of sexual identity due to the absence of the father in war and to maternal deprivation, see Anna Maenchen, "A Case of Superego Disintegration," *Psychoanalytic Study of the Child*, 2 (1946): 257-62.

¹⁰⁹ Gerd Biermann, "Identitätsprobleme jüdischer Kinder und Jugendlicher in Deutschland," *Praxis der Kinderpsychologie und Kinderpsychiatrie*, 13 (1964): 213-21.

¹¹⁰ I am especially impressed by the warmth and empathetic quality of D. W. Winnicott's wartime papers and broadcasts on the BBC. See "Children in the War" (1940), "The Deprived Mother" (1939), "The Evacuated Child" (1945), "The Return of the Evacuated Child" (1945), "Home Again" (1945), all in *The Child and the Outside World*.

its mother," they write, "but certainly from the second year onward it is an integral part of its emotional life and a necessary ingredient in the complex forces which work towards the formation of its character and its personality."¹¹¹

The researchers found that absent parents were greatly idealized. Their letters were carried around and had to be read to the children innumerable times.¹¹² When the father was away in the armed services he was spoken of by his child in terms of endearment and admiration. Especially children who were in reality rejected or disappointed by their fathers formed passionate, loving, and admiring relationships to them. When a child had never known his father he would invent an idealized fantasy father who sanctioned his forbidden greedy and destructive wishes, who loved him and gave him security.¹¹³

When a father came home on leave, however, and thereby encroached on the existing close mother-child relationship, he was met with resentment and hostility by the child. The father was viewed as an intruder who separated the mother and son. One little boy said: "Do write to my Daddy, I don't want him to come here. I don't want to have lunch with him. Somebody else can have my Daddy."¹¹⁴ But the same son and his father were best of friends when they were left alone without the mother.

When in some cases the ultimate disaster struck, Anna Freud and Burlingham report a complete inability of the children to accept their father's death. All the orphaned children talked about their dead fathers as if they were still alive. They denied the fact of death with fantasies of the father's rebirth and return from heaven.¹¹⁵

The most original psychoanalytical approach to National Socialist youth, and the one that I find conceptually most perceptive and useful, is Martin Wagh's excellent analysis of 1964.¹¹⁶ He structures the psychodynamics of the First World War German children who came to the age of political effectiveness with the rise of Hitler with precision and insight. A preoccupation with guilt, Wagh points out, is also an unrecognized self-reproach for unresolved aggression against the father. Aggression toward the absent father-rival is expressed in gleeful ideas concerning his degradation and defeat. But the hostility is coupled with a longing for the idealized father that exacerbates childish homosexual wishes. These homosexual longings offer a way out of the Oedipal conflict that is heightened for sons left alone with their mothers. In these cir-

¹¹¹ Anna Freud and Dorothy Burlingham, *Infants without Families: The Case for and against Residential Nurseries* (New York, 1944), 102, 103, respectively.

¹¹² Anna Freud and Burlingham, *War and Children*, 154-55.

¹¹³ Anna Freud and Burlingham, *Infants without Families*, 108, 110, 113.

¹¹⁴ *Ibid.*, 111.

¹¹⁵ *Ibid.*, 107.

¹¹⁶ Martin Wagh, "National Socialism and the Genocide of the Jews: A Psycho-Analytic Study of a Historical Event," *International Journal of Psycho-Analysis*, 45 (1964): 386-95; see also Wagh, "A Psycho-genetic Factor in the Recurrence of War," *International Journal of Psycho-Analysis*, 49 (1968): 319-23.

cumstances the woman is often rejected, and the incestuous wish is ascribed to someone else. These mental defenses, Wagh suggests, were renewed in the Nazi movement's deification of the Führer and its infernalization of the Jew. Homosexual tension was relieved through submission to an all-powerful leader, through turning women into "breeders" of children, and by persecuting Jews as "incestuous criminals" and "defilers of the race." The passive-masochistic inclinations that develop when boys are brought up and disciplined by mothers who are anxious and punitive may be defended against by preference for submission to a man, as this is less threatening and less castrating than submission to a woman. Self-humiliation and self-contempt were displaced onto the Jews and other supposedly inferior people, thereby assuaging feelings of unworthiness and masochistic fantasies of rejection. Since the former wartime enemies were for the time being unassailable, the Jew, who was defenseless and available, became by the mechanism of displacement the victim of those who needed a target for regressive action.

This line of research has been carried on to the contemporary problem of the children of World War II.¹¹⁷ Herman Roskamp, in a clinical study of German university students born during the Second World War, emphasizes the conflict between the child's perception of the father during the war as a highly idealized fantasy object bearing his ideas of omnipotence and the way in which the father was perceived on his return in defeat.¹¹⁸ While away the father had been honored and admired; he was the object of extreme hopes and expectations upon his return. It quickly became apparent that he was not what had been longed for. Instead he was a defeated, insecure father breaking into a heretofore fatherless family. Up to this time the mother had represented all aspects of reality. The father, by contrast, was now a demanding rival who left most wishes unfulfilled, who disappointed many hopes, and who set many limits where formerly there had been none.

Among the richest sources for the expression of the experience of young Germans during the war and postwar years is the literature of the period, which more than held its place amid the cultural fecundity of the Weimar epoch. Sometimes literary expression can capture for historians the essence of a generation's experience both graphically and with a depth of emotional subtlety that cannot be conveyed by statistics or quantitative data. Many qualitative affects cannot be statistically comprehended or documented. It is possible to see, identify, and demonstrate father identification and castration anxiety without necessarily being able to computerize them. This is the appeal to the historian of both clinical insight and literary sensibility.¹¹⁹ Can

¹¹⁷ Alexander Mitscherlich, *Auf dem Weg zur Vaterlosen Gesellschaft: Ideen zur Sozial psychologie* (Munich, 1963), tr. by Eric Mosbacher as *Society without the Father: A Contribution to Social Psychology* (London, 1969).

¹¹⁸ Herman Roskamp, "Über Identitätskonflikte bei im zweiten Weltkrieg gebornen Studenten," *Psyche: Zeitschrift für Psychoanalyse und ihre Anwendungen*, 23 (1969): 754-61.

¹¹⁹ For a discussion of the advantages and techniques of using literary evidence in history see Alain Besançon, "Psychoanalysis: Auxiliary Science or Historical Method?" *Journal of Contemporary History*, 3 (1968): 149-62.

one measure or compare quantitatively, for example, the degree of suffering, mourning, loss, or rage a subject feels? For this kind of emotional evidence we must rely on that most sensitive of our cultural materials—the subjective written word of literature.

When this has been said, it is nevertheless astonishing to experience the great autobiographical pacifist novel *Jahrgang 1902* by Ernst Glaeser (1902–63), which describes the author's feelings with such intensity and pathos that it often reads more like the free associations of a patient in psychoanalysis than a novel. The critic William Soskin ranked *Jahrgang 1902* with *Sergeant Grisha* and *All Quiet on the Western Front* as one of the most significant works on the First World War.¹²⁰ This book ran through six German printings during the winter of 1928–29. It sold seventy thousand copies in Germany and was translated into twenty-five languages.

The book takes its title from the year of the author's birth, which also automatically became the year of his military-service class. The class of 1902 was not to experience the war of 1914–18 on the front.¹²¹ For that they were too young, but as Glaeser pointedly noted, "The war did not establish a moratorium on puberty." The book, he said, deals with "the tragedy of murdered minds and souls and diseased temperaments in the noncombatant social body."¹²²

As the war began the fathers left to join their regiments and the twelve-year-old boy observes that "life in our town became quieter." The boys played war games in which the French and Russians were always soundly beaten.¹²³ The fathers were sorely missed. They were quickly idealized and glorified. Glaeser describes the process of overestimation and identification with the father who is absent at war:

We thought only of our fathers in these days. Overnight they had become heroes. . . . We loved our fathers with a new sublime love. As ideals. And just as

¹²⁰ William Soskin, as quoted in Stanley J. Kunitz and Howard Haycraft, eds., *Twentieth Century Authors* (New York, 1942), 540.

¹²¹ For a sardonic expression from among the youngest class that went to war, see Erich Kästner, "The Class of 1899," in his *Bei Durchsicht meiner Bücher . . .* (Zurich: Atrium Verlag, 1946), 97–98. "We took the women to bed, / While the men stood in France. / We had imagined that it would be much more wonderful. / We were merely confirmants. / Then they took us to the army, / For nothing more than cannon fodder. / The benches at school were emptied, / Mother wept at home. / Then we had a bit of revolution / And potato chips came raining down. / Then came the women, just like they used to / And then we caught the clap. / Meanwhile the old man lost his money, / So we became night-school students / By day we worked in an office / And dealt with rates of interest. / Then she almost had a child, / Whether by you or by me—who knows! / A friend of ours scraped it out. / And the next thing you know we will be thirty. / We even passed an examination / And have already forgotten most of it. / Now we are alone day and night / And have nothing decent to eat! / We looked the world straight in the snout, / Instead of playing with dolls / We spit at the rest of the world, / Insofar as we were not killed at Ypres. / They made our bodies or our spirit / A wee bit too weak / They threw us into world history too long. / Too fast, and too much. / The old folks maintained that the time has come / For us to sow and to reap. / But wait a moment. Soon we will be ready. / Just a moment. Soon we will be there! / Then we will show you what we have learned!"

¹²² Ernst Glaeser, as quoted in Kunitz and Haycraft, *Twentieth Century Authors*, 540.

¹²³ Ernst Glaeser, *Jahrgang 1902* (Berlin, 1929), 242, 250–58.

we formerly used to express our admiration for the Homeric heroes or the figures of the Wars of Liberation by token symbols of clothing such as golden helmets of tin foil or Lützow caps, so we now also began, but in far greater measure, to turn ourselves symbolically into the idealized figures of our fathers.¹²⁴

The boys of the village went to the barber to have their hair cut in the close-cropped military style like their fathers.

We had our hair cut. Bare. Smooth. Three millimeters high. For this is how we had seen it on our fathers as they left for the front. None of them had hair to part now.

One evening late in September a group of fifteen determined boys went to the barber. We stood according to height and let the instrument pass over our heads. As the barber was sweeping up our hair with a broom an hour later, he said: "Now you look like recruits."

We were proud of this distinction and enthusiastically paid 40 pfennigs each.¹²⁵

By the winter of 1916 the privation of the war began to be felt in the daily lives of the boys. They were always hungry. There was never enough to eat. The steady diet of turnip soup became inedible. City folk bribed and bartered away precious possessions in order to get nourishing food from the farmers. The mother gave Kathinka, the maid, one of her finest blouses so that she would bring back food when she visited her peasant parents. Faithfully Kathinka smuggled butter past the gendarmes in her woolen bloomers. Field gendarmes and controllers appeared on the roads and at the stations to search travelers for contraband foodstuffs. The children developed tactics for deceiving the gendarmes and smuggling forbidden foodstuffs home. One boy would serve as a decoy to draw the gendarme's attention while the other raced home across the fields with a sack of flour or a ham.¹²⁶

This progression within two years from idealism to hunger and the struggle for survival is vividly described by Glaeser.

The winter remained hard until the end. The war began to burst over the fronts and to strike the people. Hunger destroyed our unity; in the families children stole each other's rations. . . . Soon the women who stood in gray lines in front of the shops talked more about the hunger of their children than of the death of their husbands. The sensations of war had been altered.

A new front existed. It was held by women. The enemies were the entente of field gendarmes and uncompromising guards. Every smuggled pound of butter, every sack of potatoes gleefully secreted by night was celebrated in the families with the same enthusiasm as the victories of the armies two years earlier. . . . It was wonderful and inspiring to outwit the gendarmes and after successfully triumphing to be honored by one's mother as a hero.¹²⁷

Oedipal longings were heightened for the sons left alone with their mothers during years of war. Starvation led to the mobilization of unconscious wishes

¹²⁴ *Ibid.*, 243.

¹²⁵ *Ibid.*

¹²⁶ *Ibid.*, 292, 294-95.

¹²⁷ *Ibid.*, 292-93.

for a return to the oral comforts of early mother-child units. Occasionally the prolonged hunger was broken by feasting on an illegally slaughtered pig or a smuggled goose that the father sent home from the eastern front. Then an orgy of feeding took place. Gluttony reigned and undernourished bellies got sick on the rich food. The windows had to be stuffed to keep the neighbors from smelling the meat. The adolescent boy and his mother consumed almost an entire twelve-pound goose in one night. A stolen drumstick for his girlfriend was to her the convincing symbol of love. Glaeser writes, "We scarcely spoke of the war any more, we only spoke of hunger. Our mothers were closer to us than our fathers."¹²⁸

The fathers were not present to shield the sons from maternal seduction. One young adolescent in the novel is seduced by a motherly farmer's wife with the promise of a large ham. But, much as the pangs of his stomach and his mother's pleading letters argued for bringing the ham home, he could not do it. The great succulent ham had become an incestuous object. He had earned it from the farm wife by taking her husband's place. Now he was too guilty and too anxious to permit himself and his family to enjoy it. The pangs of guilt were stronger than the pains of hunger. As if he could "undo" his Oedipal crime, the boy laid the ham on the farm wife's bed and left. He was tearful and depressed, feelings he rationalized as being due to his injured feelings because he was really only a substitute (*Ersatz*) for the husband. He climbed into bed with his boy comrade. In the stillness of the dawn they embraced, keeping each other warm, and he shared his story of seduction and sexual discovery.¹²⁹ In this episode we see fully elaborated the heightened Oedipal conflict when the father is absent, the increased guilt and fear of retribution, and finally the rejection of the woman as a sexual object and an exacerbation of adolescent homosexuality arising from the emotional effects of the war.

By the winter of 1917 the fathers had become aliens to their sons. But they were not only unknown men, they were feared and threatening strangers who claimed rights and control over the lives of their sons. They had become distant but powerful figures who could punish and exact a terrible price for disobedience and transgressions. Glaeser recounts his reaction as a fifteen-year-old to a letter from his father on the Russian front in terms of intense castration anxiety. The adolescent boy's Oedipal victory in having displaced his father would now be terribly expiated and revenged by a towering, castrating monster of his guilt-laden fantasies. Glaeser attempts to deny that his father has any legitimate claim to control over him

¹²⁸ *Ibid.*, 314, 342-44, 314. "Strange what part food now plays," noted a Hamburg educator and poet in his diary. "Every conversation turns on food. Whoever has hoarded supplies keeps it secret. Whoever gets anything hides it as if it were a crime. A pound of butter has become the object of a thousand questions and outpourings of envy. From where? from whom? how?" (Nov. 11, 1916). "Formerly, eating was a means to live, now it has become its purpose" (Dec. 18, 1917). Quoted in Ernst L. Loewenberg, "Jakob Loewenberg: Excerpts from His Diaries and Letters," *Leo Baeck Institute Yearbook*, 25 (1970): 192.

¹²⁹ Glaeser, *Jahrgang 1902*, 317-21.

at all. But his father would know where to find him and the inevitable retribution would be inexorable.

We were frightened. That was the voice of the front. That was the voice of those men who formerly were once our fathers, who now, however, removed from us for years, were strangers before us, fearsome, huge, overpowering, casting dark shadows, oppressive as a monument. What did they still know of us? They knew where we lived, but they no longer knew what we looked like and thought.¹³⁰

It is of biographical interest for the thesis of this essay that Glaeser went into emigration from Germany after 1933, living in Prague, Zurich, and Paris. In Zurich in 1939 he wrote a newspaper article condoning Hitler's policies and condemning his fellow emigrés. Within days he received a contract from a Berlin publisher. He returned to Germany and joined the war effort, becoming a war reporter for the Luftwaffe and the editor of the military newspaper, *Adler im Suden*.¹³¹

Thus, as did so many others of his cohort, Glaeser was two decades later to choose to wear a uniform and to identify with his distant and glorified father. The identification with the father who went out to war served to erase the memory of the feared and hated strange father who came home in defeat. By being a patriot and submitting to authority, the ambivalence of the young boy who gleefully observed his father's humiliating defeat and degradation was denied and expiated. Now he would do obeisance to an idealized but remote leader who was deified and untouchable.

Many of the emotions of German middle-class generational conflict in the decade after World War I were profoundly explored by Thomas Mann in his story of 1925, "Disorder and Early Sorrow."¹³² The setting is the home of Professor Cornelius, a historian, the time is during the inflation of 1923, and the social climate is filled with anxiety about loss of status, a widening gap between the cultures of youth and adults, and the deepening economic crisis that has caused a deterioration of faith in stable moral norms. Solid bourgeois ladies are now the Corneliuses' house servants while the brash young man who lives by speculation, drives a car, treats his friends to champagne suppers, and showers the children of the professor with gauche gifts of "barbaric" size and taste represents the postwar generation.¹³³

The story opens with the menu of the midday meal in which the main dish is croquettes made of turnip greens. The meatless dinner is a meager contrast to the opulent menus succulently described by Mann in *Budden-*

¹³⁰ Wir erschrecken. Das war die Stimme der Front. Das war die Stimme jener Männer, die früher einmal unsere Väter waren, jetzt aber, seit Jahren von uns entrernt, fremd vor uns standen, beängstigend, gross, übermächtig, mit schweren Schatten, erdrückend wie ein Denkmal. Was wussten sie noch von uns? Sie wussten, wo wir wohnten, aber wie wir aussahen und dachten, das wussten sie nicht mehr. *Ibid.*, 323.

¹³¹ Erich Stockhorst, *Fünftausend Köpfe: Wer war was im Dritten Reich* (Bruchsal, Baden, 1967), 155.

¹³² Thomas Mann, "Disorder and Early Sorrow," in *Death in Venice and Seven Other Stories*, tr. H. T. Lowe-Porter (New York, 1959), 182–216.

¹³³ *Ibid.*, 207.

brooks and *The Magic Mountain*. What reader can easily forget the sumptuous repasts in the restaurant of the International Sanatorium Berghof or Mann's descriptions of solid fare on the table of the patrician merchant home in the Hanseatic seaport? In the professorial home of the Weimar era the dessert is a powdered pudding that tastes of almonds and soap—an *ersatz* concoction symbolizing the current hard times and the decline in standard of living. Many people have had to give up their telephones, but the Corneliuses have so far been able to keep theirs. Repairs cannot be made on the house for lack of materials. The professor washes at a broken basin that cannot be repaired because there is nobody to mend it. Clothing is worn and turned, yet the adolescents of the family do not notice, for they wear a simple belted linen smock and sandals. They are, says Mann, by birth the "villa proletariat" [*Villenproletarier*] who no longer know or care about the correct evening dress of the middle classes or the manners of a gentleman. In fact, the professor cannot, from observing their style of dress or personal bearing, distinguish his son from his working-class Bolshevik household servant. "Both, he thinks, look like young moujiks." His children are products of the disrupted times, specimens of their generation, with a jargon of their own that the adults find incomprehensible. The young enjoy contriving to get the family extra allotments of rationed foods, such as eggs, by deceiving the shopkeepers. They function better than the old folk in a world in which money has lost its value. The generational struggle is underlined by the professor's consistent mental depreciation of his adolescent son when comparing him with other young men: "And here is my poor Bert, who knows nothing and can do nothing and thinks of nothing except playing the clown, without even talent for that!" The younger son, who is but four years old, is subject to the rages of "a howling dervish." He, who is "born and brought up in these desolate, distracted times, . . . has been endowed by them with an unstable and hypersensitive nervous system and suffers greatly under life's disharmonies. He is prone to sudden anger and outbursts of bitter tears, stamping his feet at every trifle."¹³⁴

Thus Mann pictures the dislocation of continuity between the generations of the Weimar Republic. They differ in expectations and methods of dealing with reality. In the decade since 1913, when the professor bought his home, the family has in fact been proletarianized. One of the themes of the story is their varied response, as individuals of different ages, to this fact. The old generation cannot adjust, while their children are born into the new situation and need not make any adaptation of life style. Mann has sketched superbly and for all time the psychological experience of the impoverishment of the German upper-middle class and the rebellion against the norms and values of their parents by the children of the war.

The third variety of data I wish to examine is quantitative. It is a series

¹³⁴ *Ibid.*, 182, 196, 185, 183, 204, 188.

of autobiographical essays collected in 1934 by Theodore Abel, a sociologist at Columbia University, in an essay contest offering cash prizes for "the best personal life history of an adherent of the Hitler movement."¹³⁵

In reading the essays one is often struck by their didactic quality. Some writers say outright that they are delighted to write down their experiences for the benefit of American researchers at Columbia University.¹³⁶ As the essays were solicited by a bulletin at all local headquarters of the NSDAP and by announcements in the party press, and as the writers were not anonymous, one may infer that the writers suspected that party organs would be informed of any criticism and political or personal deviance in the essays. In some cases one senses that a local party functionary may have encouraged the writers to respond to the essay contest. Some contributions bear the NSDAP *Abteilung Propaganda* stamp.¹³⁷ Many tiresomely repeat propaganda slogans about Jewish war profiteering, Red vandalism in the revolution of 1918–19, and so forth.

All these caveats notwithstanding, these nearly six hundred essays constitute a valuable historical source. In the first place it is a contemporary source. No set of interviews of ex-Nazis thirty-seven years later could possibly elicit the same material. The Abel autobiographies may be utilized, not as a statistical sample for generalizations, but as bases for theory building. They will serve as a cognitive prism for drawing attention to necessary variables of political behavior rather than as a monolithic statistical sample that can produce conclusive findings for the population of the Nazi party. They can tell us, however, what excited and stimulated the writers, what preoccupied their fantasies and imaginations, how they viewed themselves, their childhoods and homes, and their enemies. These data can then become

¹³⁵ Theodore Abel, *Why Hitler Came into Power: An Answer Based on the Original Life Stories of Six Hundred of His Followers* (New York, 1938). Republished as *The Nazi Movement* (New York, 1965). For the purposes of this study I have used the first edition. Abel stressed that style, spelling, or dramatic story value were of no consideration. What was to be considered were "accounts of family life, education, economic conditions, membership in associations, participation in the Hitler movement, and important experiences, thoughts, and feelings about events and ideas of the postwar period" (p. 3). Abel had the cooperation of the National Socialist party in gathering his data. His announcement soliciting essays was distributed to all local Nazi party headquarters and was published in the party press. Abel used 600 of the 683 manuscripts contributed to the contest for his study. He did not use those that were too brief and the 48 written by women. Fortunately for historical research the original autobiographical manuscripts were turned over to the Hoover Institution on War, Revolution and Peace in Stanford, California. Today 582 of these essays are available for research, the others having been lost. I acknowledge the cooperation of the staff of the Hoover Institution, particularly Mrs. Agnes Peterson, curator of Central and Western European Collections, for making the Abel Collection available to me. Hereafter the original autobiographies, which are in the Hoover Institution Archives, will be referred to as AC and cited according to their archival number. There are factors that should induce caution in drawing generalizations from the Abel autobiographies. The sample is self-selected, not random, suggesting that motives such as the prize money and exhibitionism may have biased it. The sample is geographically weighted toward Berlin (30 per cent) and the Rhineland, and in favor of large- and medium-sized cities rather than small towns and the countryside.

¹³⁶ AC, 31.

¹³⁷ See, for example *ibid.*, 33.

referents for further theoretical conceptualization and behavioral model building, particularly with respect to emotional connotations that are not censored by the writers because they appear to be apolitical and therefore unimportant.

The most striking emotional affect expressed in the Abel autobiographies are the adult memories of intense hunger and privation from childhood. A party member who was a child of the war years recollects, "Sometimes I had to scurry around eight to ten hours—occasionally at night—to procure a few potatoes or a bit of butter. Carrots and beets, previously considered fit only for cattle, came to be table luxuries."¹³⁸ Another man's memory is vivid in its sense of abandonment and isolation expressed in language that makes a feeling of maternal deprivation very clear.

Hunger was upon us. Bread and potatoes were scarce, while meat and fats were almost non-existent. We were hungry all the time; we had forgotten how it felt to have our stomachs full.

All family life was at an end. None of us really knew what it meant—we were left to our own devices. For women had to take the place of their fighting men. They toiled in factories and in offices, as ostlers and as commercial travelers, in all fields of activity previously allotted to men—behind the plow as well as on the omnibus. Thus while we never saw our fathers, we had only glimpses of our mothers in the evening. Even then they could not devote themselves to us because, tired as they were, they had to take care of their household, after their strenuous day at work. So we grew up, amid hunger and privation, with no semblance of decent family life.¹³⁹

A study of the Abel autobiographies focused on a sample from the birth cohorts 1911 to 1915, who were small children during the war, indicates the presence of the defensive mechanisms of projection, displacement, low frustration tolerance, and the search for an idealized father. For example, the essays of two sisters born in 1913 and 1915, whose father fell in 1915, clearly demonstrate that Hitler served as an idealized father figure for them. Their earliest memories are of their mother crying a great deal and of all the people wearing black. They relate their excitement at first hearing the Führer speak in person at a rally in Kassel in 1931. The sisters were so exhilarated that neither of them could sleep all night. They prayed for the protection of the Führer, and asked forgiveness for ever having doubted him. The sisters began their Nazi party activities by caring for and feeding SA men.¹⁴⁰

Some of the men in the Abel Collection who lost their fathers early in life and were separated from their mothers especially valued the comradeship of the SA. One such man wrote, "It was wonderful to belong to the bond of comradeship of the SA. Each one stood up for the other."¹⁴¹ Massive projec-

¹³⁸ Abel, *Why Hitler Came into Power*, 14.

¹³⁹ *Ibid.*, 14–15.

¹⁴⁰ AC, 41, 42.

¹⁴¹ *Ibid.*, 96.

tion of ego-alien impulses is evident in many of the essays. One man says that bejeweled Jewesses tried to seduce him politically with cake.¹⁴² Many of the SA men who engaged in street brawls and violence blamed others, such as the police and the Communists, for instigating the fighting and for persecuting them.¹⁴³ One man displays remarkable projection and displacement of his own murderous feelings toward a younger brother when he relates the death of that brother in an unnecessary operation performed by a Jewish doctor. "Since I especially loved my dead brother," he writes, "a grudge arose in me against the doctor, and this not yet comprehensible hatred increased with age to become an antagonism against everything Jewish."¹⁴⁴

A body of autobiographies such as the Abel Collection invites a variety of research approaches, each suited to its own ends and including quantitative computerization.¹⁴⁵ This is now being undertaken by Peter H. Merkl with the aim of discovering and conceptualizing the phases of political mobilization.¹⁴⁶ Merkl writes:

¹⁴² *Ibid.*, 61.

¹⁴³ *Ibid.*, 86, 96, 206.

¹⁴⁴ *Ibid.*, 267.

¹⁴⁵ Computerization of the emotional content of autobiographical data presents almost insuperable methodological difficulties in evaluating the nuances of individual meanings and generalizing them. So very much depends on the subjective judgment of whoever does the computer coding. A statement in an autobiographical essay that one evaluator would code as "extreme leadership or Hitler cult" may appear to another man as just normal party politics. The same may be said for "anti-Semitism with a sex angle," "political violence," and other categories. As in other areas of history, much that passes as cold impersonal statistics is underpinned by a highly relativized subjective human temperament. In many cases the findings constitute such a small number of cases that they are statistically insignificant.

¹⁴⁶ For preliminary findings see Peter H. Merkl, "Die alten Kämpfer der NSDAP—Auswertung von 35 Jahre alten Daten," *Sozialwissenschaftliches Jahrbuch für Politik*, 2 (1970): 495–518. Professor Merkl is preparing a major secondary statistical study of the Abel data on which the hypotheses of the present essay may be tested. He has already established that the sample of 582 Nazi autobiographies approximates the total National Socialist party membership in such important variables as age distribution, vocational distribution, and date of initial party membership, so that this collection of autobiographies may in these respects be used as a representative sample of the Nazi party. Merkl reports that almost one-half of the autobiographies describe a childhood economic setting of poverty. A quarter of the subjects were working instead of going to school by age fourteen. A fourth also had parents who both worked or lost their fathers early in life. Only one-sixth may be said to have enjoyed a secure, middle-class childhood. Further, the response of these people to the German defeat, occupation, border wars, revolution, and the establishment of a republic, was denial and projection. Over one-fourth of the respondents placed blame for the defeat not on the Kaiser and the German general staff, which had misled them, nor on the German social and political system that was still semifeudal in 1918, but on various versions of the "stab-in-the-back" legend. Almost three-fourths of the writers attributed the Revolution of 1918 to the treason of the Spartacus Bund, international Bolshevism, the democratic parties of the Weimar coalition, or the Jews. Factors that respondents listed as activating them to membership and political participation in the Nazi party were, in over fifty per cent of the cases, marching in uniform and demonstrations. A further one-quarter named the attraction of violent action. Nearly half of the almost six hundred writers took part in street battles and fights at mass meetings or quasi-legal frontier warfare against Poland. Another sixth stated that they only participated in deployments and violent encounters with the Communists. Only one-sixth was satisfied with conventional electoral party activity. It appears that the quest for a mode of direct and violent action was the most powerful motive impelling this group toward party membership. A fascinating view of the inner objects of the respondents

A politically violent new generation was evidently raising the storm ladders against the gerontocracy of Weimar. . . . There can be little doubt about the youthful character of the Nazi movement, a political youth rebellion of violent virulence which seems to have no equal prior to our own age. It was evidently the good luck of the Nazi movement to become the chief beneficiary of an enormous, destructive tidal wave in the ebb and flow of the generations.¹⁴⁷

THE DEMOGRAPHIC FACTORS of massive health, nutritional, and material deprivation and parental absence in Central Europe during World War I should lead the historian to apply theoretical and clinical knowledge of the long-term effects of such a deprived childhood on personality. The anticipation of weakened character structure manifested in aggression, defenses of projection and displacement, and inner rage that may be mobilized by a renewed anxiety-inducing trauma in adulthood is validated in the subsequent political conduct of this cohort during the Great Depression when they joined extremist paramilitary and youth organizations and political parties. In view of these two bodies of data for which a psychoanalytic understanding of personality provides the essential linkage, it is postulated that a direct relationship existed between the deprivation German children experienced in World War I and the response of these children and adolescents to the anxieties aroused by the Great Depression of the early 1930s. This relationship is psychodynamic: the war generation had weakened egos and superegos, meaning that the members of this generation turned readily to programs based on facile solutions and violence when they met new frustrations during the depression. They then reverted to earlier phase-specific fixations in their child development marked by rage, sadism, and the defensive idealization of their absent parents, especially the father. These elements made this age cohort particularly susceptible to the appeal of a mass movement utilizing the crudest devices of projection and displacement in its ideology. Above all it prepared the young voters of Germany for submission to a total, charismatic leader.

But fantasy is always in the end less satisfying than mundane reality. Iron-

is revealed when we see their descriptions of the party enemy. Twenty-eight per cent, according to Merkl, used categories such as "immoral," "unclean," and "subhuman." An additional 22.5 per cent described the opponent as "a faceless but powerful, treasonous conspiracy." Over one-third felt threatened by powerful foreign nations; one-fifth viewed the non-German peoples as morally and otherwise inferior; a further one-third expressed hatred for foreigners residing in Germany; one-sixth wrote in vague terms of "the international enemy of the German people," "the conspiracy," or made hostile references to international Catholicism; 28.1 per cent of the group showed strong xenophobia. It is apparent that splitting and projective mechanisms for coping with negative self-feelings and with the contemporary problems of Germany are particularly massive in the Abel sample. Merkl's computerization of the Abel autobiographies is especially valuable because it permits cohort comparisons and cross-tabulations. When the older generation of the birth cohorts 1860-1901 is compared with the birth cohorts 1902-05, only 10 per cent of the older group showed a pattern of an early enthusiasm for the war followed by disillusionment, while this was true of 34.5 per cent of the younger people in the sample.

¹⁴⁷ Peter H. Merkl, "The Pre-1933 Nazi Movement: The Abel Collection Re-examined," paper read at the regional meeting of the Conference Group on German Politics, Apr. 1, 1970, in Sacramento, Calif., p. 5; courtesy of the author.

ically, instead of finding the idealized father they, with Hitler as their leader, plunged Germany and Europe headlong into a series of deprivations many times worse than those of World War I. Thus the repetition was to seek the glory of identification with the absent soldier-father, but like all quests for a fantasied past, it had to fail. Hitler and National Socialism were so much a repetition and fulfillment of the traumatic childhoods of the generation of World War I that the attempt to undo that war and those childhoods was to become a political program. As a result the regressive illusion of nazism ended in a repetition of misery at the front and starvation at home made worse by destroyed cities, irremediable guilt, and millions of new orphans.

A return to the past is always unreal. To attempt it is the path of certain disaster. There was no glorified father who went to war and who could be recaptured in Hitler. He existed only in fantasy, and he could never be brought back in reality. There are no ideal mothers and fathers; there are only flawed human parents. Therefore, for a World War I generation seeking restitution of a lost childhood there was to be only bitter reality in the form of a psychotic charlatan who skillfully manipulated human needs and left destruction to Germany and Europe. What the youth cohort wanted was a fantasy of warmth, closeness, security, power, and love. What they re-created was a repetition of their own childhoods. They gave to their children and to Europe in greater measure precisely the traumas they had suffered as children and adolescents a quarter of a century earlier.

Franklin Roosevelt as World Leader

A Review Article by ROBERT DALLEK

JAMES MACGREGOR BURNS. *Roosevelt: The Soldier of Freedom*. New York: Harcourt Brace Jovanovich. 1970. Pp. xiv, 722. \$10.00.

HENRY L. FEINGOLD. *The Politics of Rescue: The Roosevelt Administration and the Holocaust, 1938-1945*. New Brunswick: Rutgers University Press. 1970. Pp. xiii, 394. \$12.50.

DESPITE THE APPEARANCE of a substantial body of literature on New Deal diplomacy in the last twenty-five years, we still lack a careful study of Franklin Roosevelt's foreign policy leadership. The "revisionist" and "court" histories of the subject that appeared shortly after the war were more polemical attacks and replies than attempts at balanced accounts: the interested student could choose between Roosevelt the deceitful, naive, inept author of postwar communist might and Roosevelt the principled, realistic architect of fascist defeat. Subsequent studies shifted ground and concentrated more on the broad sweep of American diplomacy than on Roosevelt's role in the conduct of foreign policy. While only one major work went so far as to describe FDR as a weak president who "did not lead or define American foreign policy during World War II," the others limited Roosevelt to the part of a world leader more the subject than the master of external events. The more narrowly focused books that appeared in the 1950s and 1960s on such subjects as neutrality, lend-lease, and Yalta, to mention just a few, also subordinated Roosevelt's role to the rush of foreign and domestic developments.¹

¹ There is a good summary of the literature on Roosevelt's foreign policy leadership in Robert A. Divine, *Roosevelt and World War II* (Baltimore, 1969). Representative revisionist works are: Charles A. Beard, *President Roosevelt and the Coming of the War, 1941* (New Haven, 1948); William Henry Chamberlin, *America's Second Crusade* (Chicago, 1950); and Charles C. Tansill, *Back Door to War: The Roosevelt Foreign Policy, 1933-1941* (Chicago, 1952). Prominent examples of "court" histories are: Robert E. Sherwood, *Roosevelt and Hopkins: An Intimate History* (New York, 1948); and Basil Rauch, *Roosevelt: From Munich to Pearl Harbor* (New York, 1950). Studies concentrating more on the diplomatic give and take than on Roosevelt's performance are: William L. Langer and S. Everett Gleason, *The Challenge to Isolation, 1937-1940* (New York, 1952); Langer and Gleason, *The Undeclared War, 1940-1941* (New York, 1953); Herbert Feis, *The China Tangle: The American Effort in China from Pearl Harbor to the Marshall Mission* (Princeton, 1953); and Feis, Churchill, Roosevelt, Stalin: *The War They Waged*

This “neglect” of Roosevelt’s “contributions to American foreign policy” led Robert Divine to publish a brief volume of essays in 1969 on *Roosevelt and World War II*. A summary and reappraisal of earlier thinking on “FDR’s role in the coming of war and in the conduct of wartime diplomacy,” the book provided a concise treatment of Roosevelt’s foreign policy leadership. In it Divine argued that Roosevelt’s “diplomacy served the nation well,” with “his role in insuring the downfall of Hitler . . . alone enough to earn him a respected place in history.” On balance, though, Divine rendered a negative judgment on the president’s performance, concluding that “Franklin Roosevelt’s claim to greatness must rest on his achievements in domestic affairs. His conduct of foreign policy,” Divine said, “never equaled his mastery of American politics and his ability to guide the nation through the perils of depression and war.”

Divine explained Roosevelt’s “failure as a world statesman” as a consequence of misleading convictions: his commitment to isolationism in the mid-thirties, which encouraged American pacifism and foreclosed the United States from a role in preventing aggression; his “deep-seated aversion to war . . . [which] paralyzed his foreign policy” between 1939 and 1941 and “left the decision for peace or war in the hands of Hitler”; his belief in the responsibility of great nations for peace, which helped produce a United Nations under the control of the great powers; and his attachment to short-run, “pragmatic” dealings with the Russians, which provided no basis for postwar Soviet-American accord.

All of Divine’s conclusions, as he himself indicated, had been voiced before. Other scholars had also complained of Roosevelt’s hesitant prewar leadership, assigned him some responsibility for the cold war, and described his ideas about collective security as distinctive from Wilson’s. Divine departed from these earlier interpretations, however, when he argued that a sincere commitment to isolationism rather than domestic political considerations stood behind the president’s cautious prewar diplomacy and that a reliance on flexibility and realism rather than personal charm marked his approach to the Russians. Still, despite these differences, Divine’s book was more a highly useful restatement of earlier points than a searching reappraisal of Roosevelt’s direction of foreign affairs.²

The appearance of James MacGregor Burns’s *Roosevelt: The Soldier of Freedom*, the second volume in his biography of FDR, allows us once more to confront the problem of Roosevelt and foreign policy. Professor

and the Peace They Sought (Princeton, 1957). Gabriel Kolko, *The Politics of War: The World and United States Foreign Policy, 1943–1945* (New York, 1968), 348–50, minimizes Roosevelt’s role in wartime planning. A few examples of works focusing more on specific policies or episodes than on Roosevelt’s leadership are: Robert A. Divine, *The Illusion of Neutrality* (Chicago, 1962); Warren F. Kimball, *The Most Unsordid Act: Lend-Lease, 1939–1941* (Baltimore, 1969); John L. Snell, ed., *The Meaning of Yalta* (Baton Rouge, 1956); and, more recently, Diane Shaver Clemens, *Yalta* (New York, 1970).

² The above quotes and conclusions are taken from *Roosevelt and World War II*, ix–x, 3–6, 21–22, 43, 46–50, 72–73, 97–98.

Burns's book, of course, is not strictly a study of presidential diplomacy. It is a brilliant portrait of Roosevelt the "thinker," "organizer," "manipulator," "strategist," "idealist," and man. At the same time it is a history of vast proportions: the transformation of the presidency, the election of 1944, the growth of union power, the treatment of blacks and ethnic groups, the Manhattan Project, military planning—Atlantic First, Torch, Overlord—and the wartime meetings at Argentia, Casablanca, Tehran, and Yalta. The book integrates these great political and social alterations, strategic decisions, and diplomatic conferences into the study of FDR's presidency with a freshness and balance that makes it the premier study of Roosevelt and America in a world at war.

Still, to a large extent, the book is about Roosevelt's leadership of the nation in its dealings with other states. As such it presents a sharply critical portrait of Roosevelt as "a deeply divided man—divided between the man of principle, of ideals, of faith, crusading for a distant vision, on the one hand; and, on the other, the man of *Realpolitik*, of prudence, of narrow, manageable, short-run goals, intent always on protecting his power and authority in a world of shifting moods and capricious fortune" (p. vii).

Burns sees this duality in Roosevelt as playing havoc with his noblest aims. In the thirteen months between the election of 1940 and Pearl Harbor, for example, although the president stood committed to saving Britain from Nazi might, he allowed domestic political considerations to deflect him from this goal. Indeed, his reluctance to challenge isolationists was so great that he failed to take advantage of his "capacity to mobilize influence in national politics," which "was probably greater in early 1941 than . . . at the height of the euphoria of 1933." Stated another way, he did not take into account opinions on foreign policy that were "shrill, divided, inchoate and waiting on leads." He did not throw "his full weight into the congressional struggle" over lend-lease, compromising on several major amendments and using his direct influence sparingly. On protecting hard-pressed British shipping, he refused in the spring and summer, in spite of apparent majority support, to order American escorts, waiting instead for a major provocation from Berlin. Consequently, by November—despite commitments to defend Iceland, convoy British vessels, shoot at German and Italian craft in defensive waters, and arm American cargo ships—the country seemed stalled and unable to take the final step toward war. The "immediate, proximate reason lay with the President of the United States," who "had been stranded midway between his promise to keep America out of war and excoriation of Nazism as a total threat to his nation. . . . He had lost the initiative; now he could only wait on events" (p. 149).

Roosevelt's wartime leadership gets equally low marks, with expediency and the "vice of immediacy" again playing him false. Placing his emphasis on "'winning the war'—that is, gaining a military victory as quickly and as inexpensively as possible," Roosevelt saw little room for purposeful govern-

ment action on emerging, long-term domestic problems. "The burning cities of 1967 and 1968," Burns says, "were not wholly unrelated to steps not taken, visions not glimpsed, priorities not established in the federal agencies of 1943 and 1944" (pp. 354–55).

Similarly, long-run international goals repeatedly gave way to short-run need. Here, however, visions were glimpsed and enunciated "with unsurpassed eloquence and persistence": to eradicate fascism and win Soviet friendship for postwar cooperation; to turn a divided, weak China into a great democratic power; to transform Asian colonies into self-governing states; and to make international peacekeeping the function of a new league. Although sincerely meant, each of these lofty plans fell victim to pressing wartime demand: gaining Soviet trust mattered less than saving American lives through delays in a cross-channel attack; the massive military aid and political pressure required to make China into a strong, democratic state could never be found or applied; abolishing Asian colonies commanded less commitment than did good relations with Western Allies; and an effective postwar federation of states took second place to more practical arrangements among the big powers for keeping the peace. In sum, Roosevelt may have preached lofty ends, but he practiced limited means. And the result was a gap between popular expectations and possibilities, which in turn produced disillusionment and cynicism at home and poisoned relations with Russians and Asians abroad.

To Burns, Roosevelt's cautiousness, his inclination to side-step great issues and to follow the expedient rather than the bold, diminished his stature as president. Indeed, although Burns makes much of Roosevelt's greatness, of his successful struggle against the enemies of peace and democracy, and of his accomplishments as a "soldier of freedom," he is at bottom pointedly critical of Roosevelt the war leader—the man who refused to appeal directly to the people on great developing issues, who "spent far more time feinting and parrying in everyday politics than in mobilizing the country behind crucial decisions," who failed to find the intermediary means between global ideals and national interests, and who ended by sacrificing long-term schemes to short-run success.

One may raise a number of questions about Burns's picture of Roosevelt's leadership. First of all, is his suggestion that the president would have done better to offer bolder leadership in 1941 in line with the facts? The answer depends on the interpretation one gives to public opinion. Burns shows us that divisiveness, confusion, and instability were characteristic (as they usually are) of mass thinking about foreign policy. More specifically, mass opinion at this time was favorable to all-out aid to Britain even at the risk of war and at the same time firmly opposed to any involvement in the fighting. Burns also reminds us of the sharp differences between leaders of isolationist and interventionist opinion and of the more subtle divisions within each camp. Such circumstances, Burns suggests, were tailor-made for presi-

dential leadership: "amid the impenetrable events of early 1941 people seemed to be waiting for some clarifying event or galvanizing incident—or at least for some clear lead from the top. Only the President could give such a lead" (p. 99).

One may put a quite different construction on these facts. If the state of the public mind (or, more precisely, the states of mind of the various publics) was what Burns represents it to be, it may be argued that Roosevelt's restraint showed excellent political sense. With the growing external threat and the division in the country's leadership arousing the usually inattentive majority, a direct appeal to the people from the president might have fixed mass attention more fully on the crisis and activated fundamental isolationist beliefs. Students of public opinion have shown us that in unsettling circumstances involving foreign affairs most Americans seek assurance in an apparently incontrovertible truism.³ In 1941 this truism, or fundamental mood as Burns calls it, "took form in a simple, powerful, irresistible feeling against taking part in foreign wars. Defense, yes; aid to the Allies, perhaps; but foreign wars—never" (p. 42). Stated another way, in 1941 Americans seemed to hold an image of their country as above Europe's petty quarrels and wars, or as a nation offering the world an example of a country too sensible to fight. Moreover, as Burns and others have pointed out, Roosevelt helped create this mood in the 1930s, capping it in the 1940 election with promises to keep the country free from foreign wars. How the president could have successfully reversed himself in 1941 and persuaded Americans of the need to give up an apparently cherished belief Burns does not say. Nor does he explore the possibility that an appeal to the people might have made it more rather than less difficult to speed aid to Britain and get a decision for war.⁴

All such conclusions about public opinion must be acknowledged as mostly speculative, for Burns's reconstruction of public feeling on the great foreign policy issues of 1941 is quite incomplete. His reliance on opinion polls and the notebooks of Hadley Cantril, the psychologist and pioneer pollster, to the near exclusion of White House mail, congressional opinion, isolationist and interventionist elites, and newspapers leaves us with a good quantitative

³ On mass attitudes toward foreign policy, see Gabriel A. Almond, *The American People and Foreign Policy* (Praeger ed.: New York, 1960), 5–6, 23–33, 53–54, 80–84; V. O. Key, Jr., *Public Opinion and American Democracy* (New York, 1961), 173–74, 215, 256–59; and James N. Rosenau, *Public Opinion and Foreign Policy* (New York, 1961), 35–39. For a discussion of mass response to more recent events in which "primitive" feelings about foreign affairs were aroused, see Robert Rothstein, "Domestic Politics and Peacemaking: Reconciling Incompatible Imperatives," *Annals of the American Academy of Political and Social Science*, 392 (1970): 62–75. For a summary of the debate over whether public opinion imposes a constraint upon the president's conduct of foreign policy during periods of relative calm, see Kenneth N. Waltz, *Foreign Policy and Democratic Politics: The American and British Experience* (Boston, 1967), ch. 10.

⁴ Theodore A. Wilson is instructive on this point, showing that Roosevelt hoped to use the Atlantic Conference "to pull 'forward' public opinion," but instead it only made "interventionists and isolationists more vociferous in their demands. . . . Far from easing his difficulties, the meeting pinned F.D.R. tightly between the horns of isolationist denunciation and interventionist outcry." *The First Summit: Roosevelt and Churchill at Placentia Bay, 1941* (Boston, 1969), 264–65.

but poor qualitative portrait of opinion. As a result we can now say a good deal about how many people lined up on one side or another of an issue but little about how salient it was to them. The importance of such information has been readily demonstrated: a Gallup poll in October 1941, for example, showed that seventy per cent of the population believed it more important to defeat Hitler than to stay out of the European war. Roosevelt refused to take the poll at face value. In possession of a large number of letters from mothers who did not want their sons in a war, the president appreciated that "for some of the population, opposition to any interventionist move was clearly an opinion high in saliency. While Roosevelt may well have accepted the estimate that 70 per cent of the population leaned one way, he could hardly jump to the conclusion that effective public opinion followed the same pattern." The point is not that Burns failed in his reconstruction of prewar opinion—even the most careful student of that problem might fall short—but rather that his assumption of a public ready to follow bolder leads is clearly questionable.⁵

Burns can be more justifiably criticized for not giving us a fuller account of how Roosevelt judged public opinion. To be sure, Burns has much to say about FDR's concern with public attitudes, showing us that he took regular soundings from visitors, opinion polls, White House mail, fellow politicians, and newspapers, and that he "used polls more systematically than was realized at the time." While this adds to what we previously knew about Roosevelt and public opinion, it still does not tell us why the president saw the isolationists as continuing to hold so strong a grip on the public mind. Was there support for this interpretation in the information he received, or was his

⁵ While we have accumulated a good bit of information about the state of public feeling toward foreign policy in the years 1940–45, it has not been integrated into a comprehensive study that generalizes confidently about shifts in opinion or about what response Roosevelt would have met had he provided bolder leadership at any given time. Printed sources for such a study would include: Hadley Cantril, ed., *Public Opinion, 1935–1946* (Princeton, 1951); Mark Chadwin, *The Warhawks: American Interventionists before Pearl Harbor* (Norton ed.; New York, 1970); Wayne S. Cole, *America First: The Battle Against Intervention, 1940–1941* (Madison, 1953); Cole, *Senator Gerald P. Nye and American Foreign Relations* (Minneapolis, 1962); Raymond H. Dawson, *The Decision to Aid Russia, 1941: Foreign Policy and Domestic Politics* (Chapel Hill, 1959); Robert A. Devine, *Second Chance: The Triumph of Internationalism in America During World War II* (New York, 1967); David B. Johnson, *The Republican Party and Wendell Willkie* (Urbana, 1960); Walter Johnson, *The Battle Against Isolation* (Chicago, 1944); Manfred Jonas, *Isolationism in America, 1935–1941* (Ithaca, 1966); Kimball, *The Most Unsordid Act*; Andrew J. Schwartz, *America and the Russo-Finnish War* (Washington, 1960); Arthur H. Vandenberg, Jr., ed., *The Private Papers of Senator Vandenberg* (Boston, 1952); H. Bradford Westerfield, *Foreign Policy and Party Politics: Pearl Harbor to Korea* (New Haven, 1955); and Roland Young, *Congressional Politics in the Second World War* (New York, 1956). For some of the difficulties involved in trying to reconstruct past opinion, see Ernest R. May, "An American Tradition in Foreign Policy: The Role of Public Opinion," in William H. Nelson, ed., *Theory and Practice in American Party Politics* (Chicago, 1964), 100–21, from which the example of how Roosevelt made qualitative distinctions in opinion is borrowed (p. 119). An example of how one can deal with the difficulties May discusses is: Ernest R. May, *American Imperialism: A Speculative Essay* (New York, 1968). Other suggestions on how to reconstruct past opinion can be found in Robert A. Kann, "Public Opinion Research: A Contribution to Historical Method," *Political Science Quarterly*, 73 (1958): 374–96; and Lee Benson, "An Approach to the Scientific Study of Past Public Opinion," *Public Opinion Quarterly*, 31 (1967–68): 522–67. Neither of these articles, however, addresses itself to the specific question of past attitudes toward foreign policy.

assumption chiefly the expression of a preconceived idea? It is clear that the polls revealed a divided and often confused public and that congressional isolationists gave him cause for concern. But what did White House mail show? If, as one scholar suggests, Roosevelt used his mail as the "equivalent of the social researcher's qualitative interviews," it may have been the prime source of his conviction that strong interventionist support would be lacking in a showdown. But this is unclear, and without a better idea of how FDR determined the state of public belief it is difficult to speak convincingly about his leadership.⁶

Other questions can be raised about Burns's critique of Roosevelt's leadership. Is it so clear that Roosevelt's failure to create an earlier second front was the single most important factor in producing the cold war? Burns's suggestion that the second-front delay was central to postwar Soviet-American troubles can be answered with the query: Would an earlier, less successful or unsuccessful European attack have quieted Soviet suspicions of the West? Failure would certainly have brought forth a new round of Soviet complaints, and even a successful cross-channel attack in 1942 or 1943 was no hedge against the cold war. The Soviets, according to Adam B. Ulam, were not easily dissuaded from "their suspicions about the intentions of the Western Powers. Not the most intensive credits, not even the turning over to the Russians of sample atomic bombs could have appeased them or basically affected their policies. Suspicion was built into the Soviet system. . . ." While revisionists would not subscribe to Ulam's point, they, along with most other writers on the cold war, see the story as much more complicated than Burns's second-front thesis suggests.⁷

Furthermore, it is not so transparent that Roosevelt could have arranged an earlier cross-channel assault. British opposition and want of military means, particularly landing craft, made a pre-1944 attack difficult to undertake and unlikely to succeed. Such a campaign would not only have cost American lives, it would also have played havoc with the president's entire war strategy,

⁶ For Roosevelt's use of White House mail, see Leila A. Sussman, "FDR and the White House Mail," *Public Opinion Quarterly*, 20 (1956): 5-16. Theodore Wilson argues that in judging public reaction to the Atlantic Conference Roosevelt relied chiefly on opinion polls, giving little attention to letters and editorial clippings. *First Summit*, 265-66. In analyzing Roosevelt's perception of the reaction to the Quarantine Address, Dorothy Borg shows the president and other high administration officials paying greater heed to critical than supportive response, all but ignoring the written expressions of approval that formed the bulk of Roosevelt's mail. *The United States and the Far Eastern Crisis of 1933-1938* (Cambridge, Mass., 1964), 386-98. For general discussions of how little we know about what government officials see as public opinion or the role it plays in their foreign policy deliberations, see May, "American Tradition in Foreign Policy," 112-21; and Bernard C. Cohen, "The Relationship between Public Opinion and Foreign Policy Maker," in Melvin Small, ed., *Public Opinion and Historians* (Detroit, 1970), 65-80. It is Cohen's conclusion that foreign policy leaders perceive their freedom of maneuver with the public "more accurately than many scholars have" (p. 79).

⁷ Adam B. Ulam, *Expansion and Coexistence: The History of Soviet Foreign Policy, 1917-1967* (New York, 1968), 399. Ulam's conclusion is generally shared by Arthur Schlesinger, Jr., "The Origins of the Cold War," *Foreign Affairs*, 46 (1967): 22-52. The revisionist case is made in Gar Alperovitz, *Atomic Diplomacy: Hiroshima and Potsdam* (New York, 1965), and in his *Cold War Essays* (Anchor Books ed.; New York, 1970).

undermining the nation's ability to break German and Japanese power as quickly and as inexpensively as it did. Burns gives Roosevelt high marks as a strategist, calling him "an architect of military victory." Would an earlier second front have compelled a less positive description? Burns does not say.⁸

A similar point can be made about Burns's view of Roosevelt's China policy. Showing us that Roosevelt committed himself to a strong, effective China contributing to Japanese defeat and postwar stability, Burns sees Roosevelt frustrating his own goal by failing to put the military and political means behind such a policy. It is an argument with which others, including Tang Tsou, the author of the fullest account of America's wartime and postwar failure in China, have agreed. Such an argument, however, is open to challenge on two grounds. First, had Roosevelt shifted priorities and extended massive aid to the Chinese, it would have been at the expense of Europe and of island-hopping in the Pacific, an unhinging of the balanced strategy that brought so swift a conclusion to the war. Second, but more important, it is impossible to show that Roosevelt could have worked his will in China. The obstacles, as he apparently came to appreciate, were too great for him to overcome: a government set against political reform, a runaway inflation unresponsive to outside control, an ineffective army too divided to make strong, and a head of state unwilling to follow foreign leads. Alongside Chinese traditions and conditions, of what consequence was American aid and pressure? Probably, none. "China was a problem for which there was no American solution."⁹

Other Asian lands were even more remote from American influence. Although independence for Burma, India, and Indochina may have been one of Roosevelt's goals, only the Philippines, where he in fact had a final say, could benefit from Roosevelt's commitment to self-rule. Even with "a carefully conceived strategy" and a predisposition to challenge Atlantic allies, Roosevelt lacked the tools to control Asian affairs. As Burns himself makes clear, FDR could not easily budge Churchill or de Gaulle on matters considered exclusively in their own sphere. What, then, would it have taken to force Britain and France from their Asian colonies? And were Americans prepared to pay the price in tensions with their allies and obligations to the emerging states? Burns does not explore these points. Nor does he take account of the immediate postwar American experience in Korea, where the burdens of but one former colony became more than we cared to bear.

The United Nations is a final case in point. Burns complains that despite Roosevelt's central concern with a strong postwar league he agreed to a

⁸ For a summary of the problems with an earlier second front and Roosevelt's overall performance as commander in chief, see Kent R. Greenfield, *American Strategy in World War II: A Reconsideration* (Baltimore, 1963).

⁹ Tang Tsou, *America's Failure in China, 1941-1950* (Chicago, 1963). The quote is from Barbara W. Tuchman, *Stillwell and the American Experience in China, 1911-45* (New York, 1971). Dean Acheson argues the same point in his "Letter of Transmittal," in *The China White Paper: August 1949* (Stanford ed.; Stanford, 1967), iii-xvii.

United Nations Security Council that was weakened by great-power control of peacekeeping. Believing the president was too conscious of Congress and of Wilson's defeat, Burns sees Roosevelt as unmindful of both the chance to lead Americans to accept the policies and mechanics he thought necessary to postwar peace and the opportunity to compete successfully with Nazis and communists for world allegiance to his plans. Again the question is one of domestic and foreign constraints, and again the evidence runs counter to Burns's picture of Roosevelt's freedom to lead. For, as Burns himself relates, at the same time that discussion of a permanent world organization raised questions that "aroused disturbing echoes of the controversies that had done the League to death," presidential adversaries on the Hill held power to deny American participation in a new postwar league. Moreover, the fact that the country's "internationalist opinion had little intellectual basis," making it prone to fickleness and skepticism as events changed, must also have given Roosevelt pause; so unstable a body of opinion would likely prove unreliable in a showdown fight for a league. Finally, Russia's suspiciousness of other states and her power to sabotage a new peace organization gave Roosevelt little room to maneuver around a big-power veto in the Security Council. A limited United Nations working to bring Russia into the comity of nations was, in Roosevelt's judgment, better than no UN at all. As with the second front, China, and Asian colonies, the president, contrary to Burns's view, lacked the means to achieve his idea of a UN.

This failure on Burns's part to give sufficient weight to the constraints operating on FDR is made particularly apparent through a reading of Henry Feingold's *The Politics of Rescue*. Feingold's book is a sharp critique of the Roosevelt administration's failure, from 1938 to 1945, to do all it could to rescue the Jews from Hitler's annihilation program. Here, in less emotional though equally cogent fashion, is the story Arthur Morse told in *While Six Million Died*, a story of political calculation, red tape, and State Department anti-Semitism that barred Jewish migration to the United States.¹⁰ It is a depressing record, revealing Roosevelt and some of his subordinates at their worst. Unwilling to shoulder the domestic political risks involved in a vigorous rescue effort, or, once the United States entered the fighting, to take issue with the idea that rescue meant diverting resources from the full-scale prosecution of the war, Roosevelt left the matter to State Department obstructionists like Breckinridge Long, who made it certain that the administration would give little more than lip service to the rescue of the Jews.

What sets Feingold's book off explicitly from the Morse study and more generally from the Burns volume is his appreciation of the limits within which Roosevelt had to work. "An effective rescue effort," Feingold tells us, "required not only inordinate energy and will but also coordination with other nations and agencies" (p. 305), and, as he shows in rich detail, this was

¹⁰ Arthur D. Morse, *While Six Million Died: A Chronicle of American Apathy* (New York, 1967).

not easily within Roosevelt's grasp. The congressional restrictionists, the British, the Vatican, the Latin Americans, the neutrals, the Arabs, the exiled governments, the conquered Europeans, the Committee of the International Red Cross, and American Jewry itself directly and indirectly threw up obstacles to effective rescue that would have inhibited even the most determined administration effort. More important, Washington could not have weakened Nazi determination to liquidate the Jews. "When the Nazi authorities could no longer doubt that they had lost the war, the cattle cars rolled to Auschwitz as if they had a momentum of their own"; deterring Berlin from the slaughter required "a miracle [that] was never in the power of the Washington policy makers" (p. 307). Yet, as Feingold also makes clear, if a greater American effort would not have produced mass rescue, it might at least have saved many more thousands of lives. Feingold's book, then, is neither an apologia for nor an indictment of the Roosevelt administration, but rather a balanced and persuasive account of what it did and might have done about rescuing Europe's Jews.

Burns's failure to reach more balanced conclusions about Roosevelt's leadership can be explained by reference to his other works. In his first Roosevelt volume (*Roosevelt: The Lion and the Fox* [1956]) and his subsequent analyses of the presidency (*The Deadlock of Democracy* [1963] and *Presidential Government* [1966]) Burns argued for American political reform through creative presidential leadership. In Burns's view the traditional, prudent, Madisonian politics of government by checks and balances has, despite occasional vigorous Hamiltonian leadership such as FDR's, produced drift and deadlock in meeting the pressing problems of our time—"urban decline, conservation, tax reform, medical care, [and] governmental organization," to cite just a few. To break the deadlock, Burns counsels a shift to the Jeffersonian model of government: the system of "majority rule embodied in a responsible party . . . educating, arousing, leading and sometimes slowing up the people." The key to such a transformation is a Jeffersonian leader: a man who "must gain leadership of a big national party and guide it in seizing and holding majority status. He must publicize his and his party's program and goals with such clarity and conviction that he can help convert latent and amorphous popular attitudes into a powerful public opinion bolstering his cause. . . . He must be willing in emergencies to take sweeping action, no matter how controversial, and then to appeal to the electorate for a majority. . . . He must be a constructive innovator, who can re-shape to some degree the constellation of political forces in which he operates. To reach the acme of leadership he must achieve a creative union of intellectual comprehension, strategic planning, and tactical skill, to a degree perhaps not paralleled since Jefferson."¹¹

¹¹ *Roosevelt: The Lion and the Fox* (New York, 1956); *The Deadlock of Democracy: Four Party Politics in America* (Spectrum ed.; Englewood Cliffs, 1963); *Presidential Government: The Crucible of Leadership* (Boston, 1966). The quotes are from *Deadlock of Democracy*, 3, 337-38.

These earlier writings suggest that the picture of a faulty leader emerging from the pages of *Roosevelt: The Soldier of Freedom* has less to do with missed opportunities in the circumstances of World War II than with a president who falls short of the Jeffersonian model Burns holds so dear. Indeed, Burns has given us a brilliant historical reconstruction marred by questionable conclusions that spring from an idealized conception of the presidency.

One can imagine a different portrait of Roosevelt as world leader, a portrait in which his caution is an asset in the successful management of one of the great transformations of American history—the shift from isolationism to internationalism, from an inward-looking to an outward-oriented nation. *In this reconstruction the president's avoidance of confrontation politics* allows isolationist sentiments to erode naturally under the weight of events, while his focus on wartime rather than less certain postwar goals prevents post-1945 disillusionment from producing another inward turn. The Roosevelt of this portrait is not the farsighted statesman of some laudatory works, but the shrewd political leader bound by his own times: he is not the willful president of a cold-war America with an unprecedented consensus for international operations, but the flexible chief executive moving step by step with a changing America poised between the old isolationism and the new internationalism; he is not the head of a nation traditionally given to planning its foreign involvements, but the president of a people accustomed to unscheduled, limited excursions in external affairs. It is the portrait of a leader who understood America's resistance to rapid change and the limits of his and the nation's power. It is the story of a president that is rich in elementary lessons for our own times.

Reviews of Books

GENERAL

J. H. PLUMB. *The Death of the Past*. Boston: Houghton Mifflin Company. 1970. Pp. 153. \$5.00.

SIDNEY POLLARD. *The Idea of Progress: History and Society*. New York: Basic Books. [1969.] Pp. x, 220. \$5.95.

ARTHUR MARWICK. *The Nature of History*. New York: Alfred A. Knopf. 1971. Pp. x, 346, xxiii. \$8.95.

There was a time when only inaugural and presidential lectures suggested that old historians never died but simply became historiographers. Now novitiates, scarcely weaned, turn historiographers overnight by engaging in what surely must be the most profitable device since cony-catching. Assembling a dozen essays, they thrust into the hands of the ignorant what a dozen assorted mentalities have said about a topic, a topic obscured rather than clarified by such historiographical shuffling. No sweat, no tears, no contribution to knowledge—just unearned increment on other men's labors. Admittedly the three books here noticed, all by Englishmen, do not warrant so sour an introduction, since they raise, depending on the bias of the reader, a variety of fundamental questions related to what has become a major industry, permeating, sometimes polluting, all others—namely historiography. Presumably because some is good, a lot is better. In the light of such addition one paradoxical question is worth considering. Why do twentieth-century historians write as if nineteenth-century historians had never lived? Even when they refer to the latter they depend on intermediaries. Should not historians of all people know what their predecessors have thought and written not only about their own little acre but also about the most complicated

and demanding theme in intellectual history, the history of history itself? How otherwise will history avoid becoming ahistorical?

The Death of the Past, three lectures delivered with characteristic allusiveness and erudition at City College, New York, by one of the most lively and prolific historians of our day, seeks to close that cul-de-sac. Skillfully weaving other men's learning and his own thought and experience, J. H. Plumb distinguishes between the past that he, with the philosophes before him, regards as a nightmare, and history that "increases man's awareness of himself" and so leads to emancipation from the past. The euphoria of the platform is manifest throughout and prompts still another question: what does euphoria say? Like the song of the Sirens it is seductive, and its theme, pervasive and persuasive, is the necessity to restore belief in progress in order "to sustain man's confidence in his destiny, and create for us a new past . . . that will help us achieve our identity . . . as men." Is there not another paradox implicit in the question that must be asked? Will that "new past" accomplish such an end more satisfactorily for our descendants than the past we are charged to discard has for us?

Whereas Plumb would restore belief in progress, Pollard, an economic historian, traces the evolution of the idea of progress and integrates it with the sense of history. In his view belief is not only necessary, but it exists, and as such provides historians with a pattern. For the most part then he narrates what the heroes of the Enlightenment and the nineteenth century thought, and how and why their ideas represented the society in which they lived. In short this is a recitation of familiar opinions, sprinkled with judgments, some of them question-

able, for instance the much too exclusive identification of progress with the new science and, more particularly, the dismissal of Epicurean visions as individual poetic explorations. Lucretius thought in terms of humanity, and very historically, which may indeed account for his revival during the Enlightenment. Substituting the mantle of philosopher for narrator and recognizing the dilemmas of today, Pollard too is convinced of the necessity to assert belief in progress because the doubters and pessimists, to whom he gives a chapter, however salutary their warnings, must lead us into the "mythology of nihilism." Is that conviction yet another mirror to decline and fall?

Three times as long and rather more matter of fact than Plumb's lectures, *The Nature of History* is equally concerned with the necessity and use of history, and with historiography as well. Although Marwick avows that "only the ignorant or the very lazy among historians refuse to read the works of their illustrious predecessors" and thereby miss the good and the bad of such examples, he is not, for all his avowal, likely to repair that gap, since the last two-thirds of his book treats the twentieth century. The product, with its engaging, even idiosyncratic, style is a superior "art and craft of history" textbook, sketching the developments of historical studies, the relation to social science, and the historian at work, all in terms of the intellectual milieu. There is wit and perception on many a page, no better illustrated than in the section on "the platitudes and clichés of history" wherein Marwick bids historians to be ever mindful of the shifting meaning of words and the burden of social science jargon. No doubt many of his judgments will excite dissent, but quite clearly he intended that.

In sum, these three books, especially those by Marwick and Plumb, are, to apply Richard Cobb's happy phrase, "second identities," not patient autobiographies but autobiographies all the same, illustrating how choice of themes and angle of vision reveal the historian and the person alike. That historians should keep on with their research is not to be denied; but they should also for the sake of others, beginners most of all, take time to tell us what they think about their subject as a whole.

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DAVID S. LANDES and CHARLES TILLY, editors. *History as Social Science*. (The Behavioral and Social Sciences Survey, History Panel. Spectrum Book.) Englewood Cliffs, N.J.: Prentice-Hall, 1971. Pp. viii, 152. Cloth \$5.95, paper \$1.95.

While different in scope from its predecessors, this is the fourth study of history as a discipline published under the auspices of the Social Science Research Council. Over the course of a quarter of a century these reports have argued the case for more explicit methodological approaches to history. The present volume is more compact, less theoretical, and, unlike its predecessors, concerned with the educational, teaching, and research facilities of the profession. Another difference is that while the first three books were polemics seeking to persuade historians, this one is also an appeal to private foundations and, particularly, the federal government for greatly increased financial support.

Since large-scale government support, such as is called for here, is only likely in America for civilian activities that appear to be socially useful, the political strength of an appeal by historians for funds inevitably depends upon establishing the standing of their discipline as a social science. The initial cost of a computerized union catalogue of works available in the United States, for example, including "articles and other parts of larger wholes" by "subject, time period, and geographical area" (p. 136) would no doubt be above a billion dollars, but perhaps less than two per cent of that year's military budget, and extensive use of the apparatus by a scholar should cost no more than a conventional type of heavy bomb. The benefits derived from such massive federal funding of regional research libraries would accrue to all scholars, however, regardless whether their personal interest was in the biological, physical, or social sciences or in other types of knowledge. The problem is obviously one of social priorities, and possibly even national defense would benefit from this diversion of funds.

To gain information on the state of things in the more favored parts of the historical profession, the six-member "panel" representing the National Academy of Sciences and the Social Science Research Council sent a thousand and one questionnaires to members of twenty-nine leading history departments, both large and small. From these the panel received a 60 per

cent response, better than the 50 per cent an SSRC committee received from one sent to seventy selected members of the profession in the early 1940s. In some respects an analysis of university and college bulletins would have yielded more exact and extensive information regarding curricula, interdisciplinary programs, and degree requirements, but it would not have revealed personal attitudes and the general lack of adequate financial support for research.

The editors argue that the present lack of money for workshop seminars, joint projects, and postdoctoral training has had effects that are often overlooked. Historians fail to exchange views, not only with social scientists, but even with other members of their own discipline. "Self-identified social historians," those presumably concerned with the broadest range of human problems, "reported the least outside training of any kind" (p. 86). Lack of funds "is similarly reflected in the individualistic character of graduate research" (p. 89).

The case for history as a social science is presented briefly, clearly, and simply. Perhaps, as a reflection of the early computer age, quantification seems slightly overemphasized in relation to qualitative or institutional conceptual schemes. Clear premises, specifically stated methods, and logical exactness are, as much as measurement, the essence of science. Graduate students who delve deeply into the uniformities of human behavior do not necessarily have to master formidable mathematical or statistical methods, although it is desirable that they should be aware of when such aids to understanding can effectively be used. For this latter reason graduate students should be taught model building and some basic quantitative techniques. As the editors lament, "methodology is the orphan of the history curriculum" (p. 82).

Since this report, including appendixes on the questionnaire, runs only to 152 pages and has much to say about the profession in general, it may be hoped that historians previously uninterested in the social sciences will give it their attention.

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CECIL J. SCHNEER, editor. *Toward a History of Geology: Proceedings of the New Hampshire Inter-Disciplinary Conference on the History*

of Geology, September 7-12, 1967. Cambridge, Mass.: M.I.T. Press. 1969. Pp. vi, 469. \$22.50.

The debt that Charles Darwin owed to his friend Charles Lyell has long been a commonplace in the story of the evolutionary epoch of nineteenth-century thought. Before it could be accepted that men had developed from lower biological forms, it had first to be understood that the earth itself had a history, an evolution of its own. Lyell's magisterial synthesis of geological ideas and observations appeared in three volumes as *The Principles of Geology* in the 1830s, just in time for Darwin to read as he voyaged round South America aboard the *Beagle*. Here was at the same time Darwin's chief inspiration and his greatest hurdle, for Lyell believed not in evolutionary development but in an endless series of changes without progress. Geology is thus the place to start for an understanding of Darwin's achievement, but the history of geology has yet to be written in a form suitable for the second half of the twentieth century. Almost all existing works treat the unfolding of geological ideas themselves, conceived less in truly historical terms than in the presentation of odd details brought forth as curiosities for the entertainment of the reader. To bring together for a week a distinguished group of geologists and historians of science "to reconstruct the intellectual climate . . . out of which the theory of evolution was to emerge" (p. 15) was therefore an exciting idea. From the New Hampshire Inter-Disciplinary Conference on the History of Geology before Darwin, held in September 1967, has come this volume of twenty-five separate essays, edited and introduced by the organizer of the conference.

The effect is hard to gauge; I suspect the book's reception will depend on whether the reader is a historian of science or a general historian—an average *AHR* reader. The former will perhaps feel that Schneer might well have omitted the first word of his title. The essays follow no common plan nor are they carefully integrated with each other, but they provide without stultifying repetition an up-to-date and relatively clear outline of the history of geology from the Renaissance to Darwin, no more difficult to assimilate than to read the standard works, now sadly out of date, by F. D. Adams, K. von Zittel, and Sir Archibald Geikie. Against this achievement the volume's failures appear

minor. The papers are arranged alphabetically by author's surname, an idiosyncrasy remedied only in part by the excellent index and by Schnee's introduction, a synthetic account in which each essay is placed in the widest possible context; I found it too brief and detailed to accomplish its intended purpose. The volume further fails to reflect the apparent format of the meetings (discussions of preprinted papers), since there is no record of discussion and only two papers show signs of revision in the light of comments. Three papers are replaced by summaries as they were (or are to be) published in full elsewhere, raising the question of the volume's unity. Nevertheless, the papers are on the whole solidly historical, free from the praise and blame for past insights distributed according to present merit that is the flaw in most earlier work in the history of geology.

The general historian, on the other hand, concerned to know how geology fits into the wider intellectual or social world, will probably be disappointed. The focus of this volume is the internal development of ideas in geology. The authors even go to some effort to deny the common view that geology and religion were closely intertwined around 1800. A. G. Werner's neptunism—based on the idea that the earth's rocks were laid down beneath a primordial ocean that has since retreated—has usually been understood as a residue of Christian belief about the Flood. Alexander Ospovat (pp. 242–56) denies any Biblical roots to Werner's thought, stressing instead the correspondence between Werner's "geognosy," the first great geological system, and the geological structure of central Germany where he worked. After echoing Ospovat's emphasis on Werner's empiricism, Leroy Page (pp. 257–71) goes on to question the notion that most geologists in the early nineteenth century followed the Rev. William Buckland and Georges Cuvier in their belief in the reality of the Flood, a notion that is central to the most historical of books in the field, Charles C. Gillispie's outstanding *Genesis and Geology* (1951). If the historian feels bewildered, shaken out of his easy complacency that geological details are both readily assimilated and trivial in the larger scheme of nineteenth-century thought, he will have to reflect on Carl Schorske's observation that the historian usually misses just those aspects of intellectual history that are rooted in

the independent tradition of the discipline itself (cf. T. S. Kuhn in *Daedalus*, 100 [1971]: 292). Schnee's volume provides the general historian who troubles himself to master its details with the materials from which to build a new synthesis between geology and other forms of thought in the age before Darwin. As one might expect of a group composed only of historians of science and of scientists interested in the history of their own field, no concessions are made in the volume under review to the historian's need for such a synthesis. If general historians ignore this important book, if historians of science, in their zeal to win the respect of working scientists, continue as they have here to ignore the broader historical issues, a central opportunity will have been lost to the history of the nineteenth century. Gillispie's twenty-year-old book badly needs successors.

Of the riches of Schnee's book, only a sample is possible in a brief review. Not only was Werner an empiricist (Ospovat, Page), but the neptunism attacked by James Hutton, his plutonist opponent from Scotland, largely antedated Werner's theories (R. H. Dott, Jr.). Werner and Hutton agreed that stratified rocks were formed under the sea, but the former believed that mountains were formed when the sea receded, the latter, that strata had been uplifted (L. G. Wilson). Hutton, guided by a deistic belief in endless cycles of growth and decay, looked to the force of internal heat to power these upheavals. After formulating his theory he proved it with the discovery of forcible intrusion of granite into older rocks. Lyell joined neptunian and plutonic forces, and he stressed as the guiding principle of geology an insight first put forward by Hutton: the processes that are forming the earth now are the same as those that acted in the past. This principle is best called "actualism" to distinguish it from "uniformitarianism"; the latter describes a steady state in which the same agencies act at the same intensities forever; under the former, the processes continue but their rates and hence their relative importance may vary over time. Lyell, restructuring geology in his *Principles* into "the last major all-explanatory system in earth science" (M. J. S. Rudwick, p. 298), found the key to his extension of geologic time in his observations of Mount Etna. In refuting the catastrophism of both neptunists and plutonists,

he denied also the transmutation of species (W. F. Cannon). Lyell, though he established actualism as geological doctrine, had an outlook chiefly uniformitarian. Though Darwin's evolutionary image of the natural world was the result of his learning to see through Lyell's eyes (p. 288), the latter was the most reluctant of Darwin's friends to support the revolutionary theory of natural selection.

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SAMUEL ELIOT MORISON. *The European Discovery of America: The Northern Voyages, A.D. 500-1600*. New York: Oxford University Press. 1971. Pp. xviii, 712. \$15.00.

This is a grand book in many ways. It is grand in conception—a saga of European voyages on and across the North Atlantic from the Irish St. Brendan in the fifth or sixth centuries to the Virginia attempts at the end of the sixteenth century. (A second volume—"God willing," says Morison—will deal with the southern voyages.) It is grand in the way of a grand tour, recounting not only historical voyaging but Morison's travels—both his purposeful cruising along the explorers' paths by sea and air, and the sojourns of a lifetime. Thus we are introduced to Jacques Cartier via an omelette enjoyed at Mont-Saint-Michel, while the French fishermen of "Terre-Neuve" are described from the deck of a steamer passing through the fishing fleet in June of 1900. It is grand in style, the Morisonian language rich, sweeping, and fittingly salty, although at times it seems the Admiral forgets which sailings he is talking about—the identification of Lord Edward Howard as "Captain R.N." and references to "shore liberty" on the American strand seem a bit too modern for the sixteenth century. And more often than not the dismissal of contrary views is too cavalier: Morison's disdain of "library geographers" vis-à-vis "historian yachtsmen," for example; of Eva G. R. Taylor for eschewing the sea she wrote about; of J. A. Williamson's argument that at least one of Cabot's ships entered the Caribbean; or of the Vinland Map that seems most guilty of being a Yale project.

One anticipates three reactions to the volume. The reading public will devour it as an immense saga of hardy men and the cruel sea—it

was upon publication a "Book-of-the-Month Club" selection. Historians of the discoveries will incorporate the work into the controversies that surround the subject. Assuredly, for example, there will be many partisans of Morison's new path for John Cabot's first voyage, and as many nit-pickers. And admittedly there are nits to pick on this and on other points, most obviously a capricious reading of John Day's letter describing Cabot's landing place where a reference to "dung" is accepted as evidence of moose or caribou while a reference to "big trees from which masts of ships are made" is ignored—the first fitting Morison's chosen spot, the latter not. But in such controversies only a few will agree that it is all a great, unending game and that the paucity of evidence precludes final solutions. One suspects that Morison is among the few: for all his decisiveness, he labels his own thought on a given subject a "sixpenny contribution." Finally, historians generally and, more specifically, the historian of discovery as the diffusion of knowledge rather than simply ship sailings—a distinction Morison does not make—will accept the volume as a grand and convenient reference work, not only of the sailings but of the literature and of sea matters in general. For among the very best parts of the book are the extensive bibliographies and notes appended à la Justin Winsor to each chapter, and the very best chapters are those on "English Ships and Seamen" and "The French Maritime Background."

DARRETT B. RUTMAN
University of New Hampshire

ALISON GILBERT OLSON and RICHARD MAXWELL BROWN, editors. *Anglo-American Political Relations, 1675-1775*. New Brunswick: Rutgers University Press. 1970. Pp. x, 283. \$10.00.

This book is the outgrowth of the Twentieth Conference on Early American History held at Rutgers to observe that university's bicentennial in 1966. One aim accomplished was to show new lines of research. Thus Professor Olson wisely emphasized the importance of studying the men who made Anglo-colonial politics work—the governors, agents, and private businessmen.

The editors of this volume are adherents of the C. M. Andrews school of thought that the colonial period of American history is Anglo-

American and that the mother country therefore must be brought to the forefront.

Of the contributors, David Williams most clearly recognizes the basic facts of the Anglo-American eighteenth-century connection. The English and Americans lived politically in "two separate worlds" set apart by great distances, tenuous communications, ignorance bred from lack of contacts, and the coming of age of a colony in a wilderness environment.

The most effective features of the book are the vignettes of various colonial governors and agents with the audacity of their voyages and efforts to maintain a working relationship and to refine governmental machinery. They vary from the portrait of a typical early eighteenth-century New York manager like Lewis Morris to William Byrd, the Virginia friend of many lesser English politicians. Lovejoy's picture of Sir William Berkeley is lifelike. Richard Dunn's characterizations of Randolph, Andros, and Albemarle are helpful, while Williams' Nicholson comes alive. Katz's study of James de Lancey and his ability to interweave his useful London and New York connections to their mutual benefit is masterful.

John Shy makes an interesting comparison of the views of two colonial governors, Henry Ellis and Thomas Pownall, emphasizing the polarization of opinion in regard to colonial policy that took place from 1763 to 1775 (p. 169). He makes the point that Pownall from his own viewpoint as a friend of America nevertheless recognized in the fourth edition of his *Principles of Polity* that Americans would not accept representation in the British Parliament, even if the British government offered it. Edmund Burke objected to Pownall's rather weak explanation that after 1763 American affairs became an object of politics, "the tools and instruments of faction." Shy's argument is convincing that if Governors Pownall and Ellis are taken to represent the limits of what was conceivable in American policy between 1763 and 1775, the range of historical possibilities was very narrow and not as wide as many have thought. Shy's contention is that Pownall's philosophy provides evidence that British colonial policy on the eve of the American Revolution was neither fortuitous nor susceptible to change.

Another of Shy's assumptions is that British

imperial policy had begun to change in many important ways early, certainly before 1750, and that these changes were merely accelerated by the Seven Years' War. Kannan's study shows that the development of English pressure groups hostile to America reduced the chance of compromise. The colonies began to be regarded by the British government in a new way—a way that Shy categorizes as the search for rationality. Shy argues that equity and efficiency were the keys to this imperial policy based on reason.

An excellent bibliographical essay by Joseph E. Illick points out that the interest in institutions and policy shown by the imperial historians of Andrews' generation emphasized gradual development and continuity whereas today's concentration on political, economic, and social roots leads to more emphasis on change and conflict.

Although this has the weakness of being a collection of essays by different scholars it contains many valuable suggestions for students of the period.

ANNE G. PANNELL

Sweet Briar College

H. E. S. FISHER. *The Portugal Trade: A Study of Anglo-Portuguese Commerce 1700-1770*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1971. Pp. xvi, 171. \$9.50.

It can be said at once that this book is likely to remain the standard work on its subject. Based on a wide range of archival and primary printed sources in England and Portugal, the author describes the impressive growth of English trade with Portugal to 1760 and its subsequent decline in the 1760s. He then analyzes in turn the four commodity trades that together accounted for an overwhelming proportion of all Anglo-Portuguese commercial transactions. These were the trade in English textiles to Portugal, whence a high percentage was re-exported to Brazil; the shipment of English and North American foodstuffs (chiefly food grains and codfish) to Portugal; the import of Portuguese wine into England; the transfer of Brazilian gold from Portugal to England.

In general the trade was chiefly conducted between London, Lisbon, and Oporto, although other ports in both countries were actively engaged and much of the gold was transferred by the monthly Lisbon-Falmouth packet boats,

which were exempt from search by the Portuguese customs officers. London-based English merchants largely controlled and financed the trade, although from the 1730s onward English merchant houses in Portugal and in the 1760s North American merchants became increasingly involved. The extent of commercial investment varied with the branch of business, but at the trade's height in the 1750s total sums involved had reached very considerable proportions. Dr. Fisher estimates that between 1700 and 1760 the generally rising imports of Luso-Brazilian gold coin and bullion into England may have totalled about £25 million in value—a sum amounting to about six per cent of all recorded imports into England during those decades. A concluding chapter discusses the relationship between the Portugal trade and the development of the English economy in the eighteenth century. The value of this carefully researched book is enhanced by a number of statistical tables and appendixes, and it is well illustrated, printed, and produced.

C. R. BOXER
Yale University

JAMES KIRKER. *Adventures to China: Americans in the Southern Oceans, 1792–1812*. New York: Oxford University Press. 1970. Pp. 192. \$7.50.

The first serious venture of the United States into the China trade began in 1792 with the French declaration of war against England and came to a temporary close with the U.S. declaration of war against England in 1812. Prior to American independence the trade of the Indian and Pacific oceans was the monopoly of the East India Company and the South Sea Company and was thus closed to American colonists. Immediately after independence the Americans found it difficult to compete in the China trade, partly because of the continued monopoly of the East India Company and partly because the Americans had difficulty in finding an effective medium of exchange for the silks, teas, and porcelain of Canton.

Britain's involvement in the Napoleonic wars changed all this. American vessels now became neutral carriers to the warring nations. The larger merchant firms took advantage of the opening of the West Indies to the Americans, and with silver bullion so obtained they sent

their ships on direct voyages to Canton, where they purchased their cargoes with Spanish dollars. The merchants of Boston and some of the smaller ports, however, used a different technique. Their ships left for the islands of the Southern Hemisphere where seal skins were obtained for the China trade. Later they discovered the considerable demand for sandalwood. It is with the voyages of the sealing ships that Mr. Ricker's anecdotal account is concerned.

And a fascinating account it is. There is nothing profound here: no searching economic or political analyses, no sweeping theories of international finance or balances of power. It is simply a series of eye-catching vignettes of those men and ships who worked their way into the South Atlantic, then both east and westward to the China coast. These are tales of the Falkland Islands, of Mas Afuera, of the sealing grounds of the Indian Ocean, of the Fijis, and finally of Canton itself. It is the story of the sealing gangs left ashore for long periods of time; of "alone men," whose ships abandoned them in remote places; of small wars fought between gangs of Yankee sailors for control of the breeding grounds. Even more hair raising are the accounts of the bloody battles in the South Pacific for the sandalwood trade and of the "beach combers" (white castaways) who organized South Pacific island tribes against each other in violent conflict.

Mr. Ricker's material is based on firsthand, eyewitness accounts by men who were in those places and did those things. His sources are carefully identified in footnotes, but unfortunately there is no organized bibliography to which one might turn for quick reference.

JOHN J. NOLDE
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Orono

NILS RUBEY. *Den nya världen och den gamla: Amerikabild och emigrationsuppfattning i Sverige 1820–1860*. [The New World and the Old: The Image of America and the Concept of Emigration in Sweden, 1820–1860]. (Scandinavian University Books. Studia Historica Upsaliensia, Number 30.) [Uppsala: Almqvist & Wiksells.] 1969. Pp. 501. 48 S. kr.

Swedish emigration studies at Uppsala University have produced this admirable volume

on attitudes toward the United States and the emigration problem in Sweden in the four decades before the American Civil War. The book follows chronologically but not substantively Harald Olovson's *Amerika i Svensk Litteratur* (1930), which covers the period from 1750 to 1820. Runeby's volume is based on an impressive array of documentary, manuscript, newspaper, and monographic materials, listed at the rear of the book and in the ample bottom-of-page citations. There is a brief summary in English, but it cannot abstract the mass of information in the book any more than can this brief review.

After an introductory survey of other treatments of Swedish and European images of America Runeby proceeds to an analysis of the literature about America and emigration from 1820 to 1840, a period in which there was virtually no emigration to the United States from Sweden. Letters of travelers to America, diaries of visitors, reports of businessmen who had American connections, consular reports, and controversies in Swedish newspapers such as *Aftonbladet* concerning the American republican experiment are examined. Beginning in the early 1840s emigration to the United States became a reality evoking strong negative reactions in Sweden. The long-drawn-out controversies in religious circles as to the dangers and rewards of emigration are fully developed by Runeby, thus filling out George M. Stephenson's notable volume, *Religious Aspects of Swedish Immigration* (1932). Runeby's book concludes with a discussion of Swedish attitudes toward the controversies in the United States in the 1850s, such as slavery and the changing nature of America. Eventually virtually all restraints on emigration were abandoned except for admonitions as to the physical dangers involved in going to America.

The author gives an effective account of the views of articulate parts of the Swedish population during the period covered. He does not, however, present to any extent the views of the mass of the emigrants as reflected in their America letters. The book is essentially a study in Swedish intellectual history as it relates to America and emigration.

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Society

DEREK HOPWOOD. *The Russian Presence in Syria and Palestine, 1843-1914: Church and Politics in the Near East*. New York: Oxford University Press. 1969. Pp. viii, 232. \$7.00.

Derek Hopwood's claim that "Orthodox Arab society was set on a path which would have been notably different had it not been for Russian intervention" may sound hyperbolic, but it is impressively supported by a detailed description of Russian activity in Syria and Palestine from 1843 to 1914. This activity was part of the general policy of St. Petersburg in raising the prestige of Russia in the Orthodox East or *Pravoslavnyi Vostok* through the use of cultural and ecclesiastical channels to supplement the traditional diplomatic agencies of Imperial Russia in the Near East. Thus, through the establishment of ecclesiastical missions under the successive leadership of such famous Russian hierarchs as Porfirii Uspenskii, Kiril Naumov, and Antonin Kapustin and agencies such as the Palestine Committee, the Palestine Commission, and more significantly the Imperial Orthodox Palestine Society, the Russians established themselves conspicuously in Syria and Palestine so that by the eve of the First World War they were considered by some as the likely heirs to that part of the Ottoman Empire in the event of the latter's dissolution. Understandably, the Russian presence aroused considerable, even though at times exaggerated, commentary in the dispatches of British and French representatives in the Levant. It was, however, a complex and frequently puzzling activity characterized by disagreements and rivalries among the Russian leaders themselves—lay and ecclesiastic—as to objectives and methods and by an atmosphere of suspicion between the Russian agents and the Greek hierarchy of the Jerusalem and Antioch Patriarchates as the Russians sought to champion the local Arab Orthodox population.

These factors often led to confusion and ineffectiveness on behalf of the Russians, but they managed to set up a great number of schools and disseminate Russian as well as Orthodox culture among the Arab Orthodox, encourage pilgrimages to the Holy Land, and generally elevate Russian interest, scholarly and popular, in the Orthodox East with which the Russians had had historical and religious ties since the tenth century.

The story in this form is not entirely new,

although Hopwood's account treats in greater detail the activities of the missions. I, among others, concerned myself with the same problem six years earlier in *Russian Interests in Palestine 1882-1914* (1963). (Despite the dates of the title, the narrative really begins, as does Hopwood's, with the preliminaries for the establishment of the First Russian Ecclesiastical Mission in Jerusalem under Porfirii Uspenskii.) Besides Russian published sources, Hopwood utilized the dispatches from the British Embassy in Constantinople and the British Consulates in Beirut, Damascus, and Jerusalem deposited at the Public Record Office, as well as Arabic published accounts. But, interestingly enough, despite some illuminating details these new sources do not alter appreciably our understanding of this subject. There is therefore a remarkable agreement between Hopwood and previous accounts as to the outcome of this Russian venture in the Eastern Mediterranean.

For me at least, one of the most useful chapters was the second, which discussed the religious, social, and economic situation of the "Orthodox World of Syria and Palestine," a subject very much in need of further investigation and for which nineteenth-century Russian travel accounts provide invaluable data. In this venture, as well as when he discusses the "dilemma of the Christian Arab," Hopwood also reflects the influence of his mentor Albert Hourani, but then moves on to trace the interest of young Russian-trained Arab teachers in Russian literary figures such as Gogol, Pushkin, Tolstoy, Chekhov, and Gorkii, some of whom they translated into Arabic at the turn of the century. This is a fascinating story that is increasingly occupying the attention of Soviet Orientalists. I wish that Hopwood had dwelt on this topic in greater detail. Still, it is Hopwood's treatment of the impact of the Russian policy—which sometimes was counterproductive as far as official Russian conservative philosophy was concerned—on the Arab Orthodox community that makes it a welcome addition to the increasing body of scholarly literature on the interests of the great powers in the Eastern Mediterranean up to World War I. American, British, and Russian interests have been treated respectively. We need a study on French in-

terests in Syria and Palestine during the same period.

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STUART L. BERNATH. *Squall across the Atlantic: American Civil War Prize Cases and Diplomacy*. Berkeley and Los Angeles: University of California Press. 1970. Pp. x, 229. \$6.50.

FRANK J. MERLI. *Great Britain and the Confederate Navy, 1861-1865*. Bloomington: Indiana University Press. 1970. Pp. xvi, 342. \$7.50.

These two monographs admirably illuminate the international dimension of the American Civil War. In view of the enormous amount of writing about that conflict it is strange that the response of the British government to Confederate efforts to acquire naval vessels in Britain has not been studied in depth. Equally strange has been the neglect of historians to study the important prize cases arising from American efforts to maintain the blockade of the South, primarily against British interlopers seeking profit of trade. Professors Bernath and Merli fill these gaps in their carefully researched, soundly reasoned, and well-written studies.

A prodigious amount of research, both in the usual and out-of-the-way places, has gone into the writing of these monographs, especially Merli's. Both volumes include bibliographical essays that are excellent guides to the study of Anglo-American relations during the Civil War. While both studies are well written, Merli's subject with its aspects of adventure and intrigue lends itself to a more sprightly presentation.

Merli writes against a broader background that includes a masterful account of Confederate naval strategy and personnel. Moreover, he gives a valuable discussion of such matters as a technical evaluation of the famous rams and the fatal defects of Confederate finance and an administration so woefully lacking efficiency and real appreciation of sea power.

But despite this extensive background, Merli never loses sight of his main theme, the response of Britain to the efforts of the Confederacy to acquire a navy in Britain and thus traverse Britain's neutral obligations. British response,

he cogently argues, was that of indifferent neutrality that pleased neither North nor South. In her unaccustomed role as a neutral maritime power, Britain had to operate under an inadequate law of neutrality in such a way as to avoid serious infringement of private interests, constitutional guarantees, and neutral obligations. To do this, according to Merli, Britain was forced to go behind and beyond the law and to do what was necessary rather than what was legal to fulfill her obligations as a neutral. This required a due regard for differences in public opinion, the political situation, the state of the Royal Navy, circumstances in Europe and British North America, and last but not least the possible needs of Britain in the future when she might be a belligerent instead of a neutral.

Bernath, in his discussion of the prize cases, also contends that Her Majesty's government anticipated a possible future use by Britain of the current American interpretation of belligerent rights and thus generally acquiesced in their enforcement despite frequent protests made in behalf of British neutral rights. The British government not only looked forward but also backward and recognized precedent for American practice in earlier British interpretation of belligerent rights that, in the case of the continuous voyage doctrine, was extended in scope by the Americans. Bernath portrays with skill a situation that was a reversal of British and American roles of the French Revolutionary era. He describes with thoroughness and clarity the incidents from which the cases arose such as the seizure of British ships and cargoes, the violation of neutral territory, the mistreatment of neutral subjects, the court proceedings and administrative maneuverings, and the diplomatic consequences that followed.

Protection of American and British national interests, at the moment and for the future, and the preservation of peace, as both Bernath and Merli make clear, required statesmanship demonstrating skill and wisdom. In Anglo-American relations the ships of state encountered vexatious squalls, but pilots like Lincoln and Seward in Washington and Palmerston and Russell in London steered them away from the hurricane of conflict. In all probability such a hurricane would have enabled the South to win

its independence, but there would have been no United States strong enough in the twentieth century to redress the Old World balance of power tilted against Britain in two world wars.

MALCOLM LESTER

Davidson College

C. L. SULZBERGER. *The Last of the Giants*. [New York:] Macmillan Company. 1970. Pp. xv, 1063. \$12.50.

This journal of C. L. Sulzberger's travels and interviews from 1954 through 1963 will often be cited, particularly the opening two chapters, which consist of selections of the most quotable *bons mots* uttered by his many interviewees. Unfortunately, clever quotations and long interviews with famous men do not a history make. I find myself in full agreement with the author's confession that he finds this "a sporadic and incomplete account." Far too long and sometimes trivial, the book is predicated on the thesis that anything said by the great and the near-great is both news and history.

How valuable is it for historians? Although some small bits of knowledge may be added to what we already know, I found no great revelations. In both the introductory chapter and the text, for example, Sulzberger treats his surmise that de Gaulle executed a coup d'état in 1958 as new and novel—which it is decidedly not. The characters we have become all too familiar with reappear in their usual grab: Averell Harriman is always the partisan politician before anything else; Dulles is the inflexible moralist; American policy makers in general are well-intentioned good men only responding to the challenge of international communism. Though the jacket claims that, freed from the restraints of the *New York Times*, this book permits Sulzberger to be "informal, unfettered, and often irreverent," this "new breed of journalist" surfaces largely in the form of quick, superficial one-liners where he gives his opinion of this statesman or that policy. Detailed research may find a few exceptions, but the essence of each important interview found its way into Sulzberger's columns. To a great degree, then, the book consists of elaborations on his columns interspersed with some political gossip and an occasional sketchy observation by the author.

The one exception may be his treatment of "The Last of the Giants," Charles de Gaulle. Again the substance of Sulzberger on de Gaulle has appeared in his books and columns, but the first six chapters—the only ones of the forty-seven that have any internal unity—present a cohesive and persuasive body of evidence. His findings are not new; de Gaulle is an anachronism from the seventeenth century. But in that first eighty-seven pages the substance of a real book appears.

The journal also provides context if not depth in helping us understand the nature of the cold war—and that is probably its lasting value. How Sulzberger, a classic example of the cold-war liberal journalist, viewed international affairs is of interest itself. His comment, for instance, "that Larry [Norstad] was the only American general . . . during the [Second World] war who was as interested in winning the peace . . . and therefore favored military operations in eastern Europe to save that area from the Russians" (p. 124), says more about the author in 1954 than it does about Norstad in 1944. More important, even though many of those interviewed spoke consciously for the public record, the overall effect after a reading of these formal talks and casual conversations is to acquire some sensitivity for the atmosphere of the cold war and understanding of the men who conduct it. The fear and suspicion that inevitably accompanies an armed truce permeates the entire book on the part of both author and subjects.

The book holds a vicarious fascination for the general reader, for it brings him into contact with the movers and doers of the world; for the researcher, fascination is not enough.

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Newark

BRUCE DOUGLASS and ROSS TERRILL, editors. *China and Ourselves: Explorations and Revisions by a New Generation*. Preface by EDGAR SNOW. Boston: Beacon Press. 1970. Pp. xxii, 259. \$7.50.

If there is one thing that distinguishes the newest generation of China specialists from their academic elders it is their acceptance of Communist rule in the People's Republic. Even in most of the sympathetic older scholars there lurks a double preoccupation: Where did the

United States "go wrong," thereby losing the fabled third force? and just how big a problem is the China Problem?

Of the ten men who composed this stimulating collection of essays, half are American (four of the others are from the Commonwealth, one is Japanese) and all are either aspiring or recent Ph.D's. Four have worked, studied, and traveled in the People's Republic. Their ideological predilections cannot be spotted—indeed, the most openly sympathetic with revolutionary China is a faithful Roman Catholic.

Stephen Fitzgerald, of the Australian National University, visited China in 1968, during the Great Cultural Revolution. In his essay he takes up the problems of why a full-scale civil war did not develop after 1966. "One answer is that the Cultural Revolution was primarily a struggle between Communists. . . . There is very little evidence to sustain the claim that there was within the Party a conspiracy working for the overthrow of communism and the restoration of capitalism." Later in his discussion Fitzgerald points out that, at least in 1968, while Mao constituted a semidivine figure for many Chinese, he appeared not to inspire fear.

Parting company with many social scientists, Jon Saari of Harvard, in discussing modernization in general and its application to China, states bluntly that the last two centuries have seen "untold brutality and violence—witness the two world wars alone—whose deeper causes lie rooted in the strains of the process of modernization." It is little wonder, therefore, that when the Chinese turned on foreign enterprises they did so like men throwing off a degrading and brutalizing dependence.

Tom Englehardt and Ross Terrill of Harvard explore two facets of America's China preoccupation: Englehardt describes the simple-minded insensitivity that could provoke an old China hand to refer to cleaned-up Shanghai as a "moral nightmare," while Terrill details the lunatic anti-communism that drove John Carter Vincent from the State Department.

Most insightful of the essayists, perhaps, is Edward Friedman, now of Wisconsin. Friedman proves himself a redoubtable seer by foretelling that "the accepted belief about 'the same old roadblock in the way: the fate of the Island of Taiwan,' is a myth. I would suggest that there is no cluster of fundamental, vital, irreconcil-

able interests separating the People's Republic of China from the United States of America." (Since Edgar Snow takes precisely the opposite line in his otherwise excellent introduction, one can only hope that now he will read Friedman.)

Friedman's essay, stressing China's great power practicality, contrasts oddly with Neale Hunter's "The Good Earth and the Good Society." Hunter, now at the Australian National University, taught in Shanghai from 1965 to 1967. He concentrates on China's "materialism," a reverence for the value of things and people. Here Hunter bumps right up against Friedman's *Realpolitik* by maintaining that in China one is "thrown back into the innocence of one's childhood or into the cultural infancy of the human race."

This may explain why Peking invited our ping-pong team.

JONATHAN MIRSKY
Dartmouth College

ANCIENT

PETER GREEN. *Alexander the Great*. New York: Praeger Publishers. 1970. Pp. 272. \$12.95.

The reader might well approach a coffee-table book written by a well-known novelist with suspicion. Potboilers on Alexander appear every year, in all major languages. A glance at the first page might confirm suspicion. "On an early September day in the year 356 BC a courier rode out of Pella . . . bearing dispatches for the King. He traveled southeast, making for Potidaea, a city of the Chalcidic peninsula, where the Macedonian army now lay; and he did not waste any time on his journey." It is pure Dumas. But let no one be deceived. This is a serious book, for the author, a Cambridge Ph.D., with his novelist's style and with the novelist's interest in character, has the erudition and the critical judgment of the historian. His Alexander is not the heroic leader or the remote philosopher-king of the German and the British mythopoeic tradition: he is the up-to-date political realist and purely military genius, the soldier-politician of flesh and blood, who has emerged from recent research.

At times Green's desire for vivid and readable presentation leads him astray into touch-

ing up the inadequate fact or relating dubious anecdote as fact—not because he does not know better. Thus, while the meetings with Indian philosophers are properly described as largely apocryphal (p. 232), the meeting with Diogenes (totally spurious) is told without even a warning to the reader (p. 75); and the Lion of Chaeronea that "still stands guard, weathered and brooding, over that melancholy plain" (p. 51) was in fact set up again not many years ago. At times Green's original historical interpretations may not find general approval, as where he accepts (with fair warning) Diodorus' account against Arrian's on the battle of the Granicus (p. 96), without really making much better sense of that battle than the orthodox version. But this at least demonstrates that the author has brought independent thought to bear on the problems. The book is well informed and excitingly written. The maps and other aids are adequate, the pictures, ranging from coins and archeological remains to medieval illuminations, are expertly selected and well (often superbly) printed. The bibliography will be of use to serious students and even to professional scholars.

E. BADIAN
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T. SULIMIRSKI. *The Sarmatians*. (Ancient Peoples and Places, Volume 73.) New York: Praeger Publishers. 1970. Pp. 267. \$10.00.

A half century has passed since the appearance of Rostovtzeff's *Iranians and Greeks in South Russia*, and during the interim archeologists have uncovered great quantities of Sarmatian artifacts. In his history of the Sarmatians Professor Sulimirski has attempted a synthesis of these data, and for this labor he deserves the thanks of both historians and archeologists.

Sarmatian is a generic term used to denote a complex of Indo-Iranian nomadic-pastoral peoples, including the Roxolani, Iazyges, and Alans. These various Sarmatian peoples roamed through Central Asia and South Russia during the millennium preceding the fall of the Roman Empire. They never formed into great hordes as did the Huns and the Mongols, and their impact upon other cultures was less intense.

The normal difficulties of writing history largely from archeological evidence (Sarmatian

culture was nonliterate) are exacerbated by the close cultural relation of the various Sarmatian peoples. As a result, when written evidence provided by observers is inconclusive, or when there simply is no written evidence, Sulimirski's attribution of a particular find to a specific ancient people raises doubts in the reader's mind. Sulimirski's assertion that the Alans were moving into the Volga steppe region during the third century B.C., more than three hundred years before they are first mentioned in the written sources, illustrates another of the author's methods that is of dubious value.

Though the written sources are not numerous, Sulimirski fails to take full advantage of them; this is particularly so when he deals with the Alans in the west, the Sarmatian people concerning whom we are best informed. There were, for example, at least six separate Alan groups in Gaul, Italy, and Spain-Africa during the fifth century and later, but Sulimirski discusses only two of them. The role of the Alans at Constantinople is all but ignored, and the great Alan generals Ardaburius and Aspar are consigned to oblivion. Sulimirski also misses some very important archeological evidence: an inscription from Vendôme bearing the Alan name Gerasp and the Central Asiatic bow-legged man motif found on Aquitanian-style artifacts. Sulimirski's failure to use the written sources adequately is exacerbated by his ignorance of recent scholarly work done on the Alans in the west.

As history *The Sarmatians* is inadequate because the author does not take full advantage of the written sources. As a guide to art and archeology the book is of considerable value.

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MEDIEVAL

M.-D. CHENU, O.P. *Nature, Man and Society in the Twelfth Century: Essays on New Theological Perspectives in the Latin West*. With a preface by ETIENNE GILSON. Selected, edited, and translated by JEROME TAYLOR and LESTER K. LITTLE. Chicago: University of Chicago Press. 1968. Pp. xxi, 361. \$12.50.

When M.-D. Chenu's *La théologie au douzième siècle* appeared in 1957 it received scant notice

from the historical journals of this country. From the perspective of more than a decade there is now no doubt that it is the leading interpretation of medieval intellectual history to have appeared since World War II and that it fully merits the present excellent translation into English. For the English version Professors Taylor and Little have selected nine of the original nineteen chapters, updated the scholarly apparatus, and translated into English the Latin texts as well. Now that Chenu's work has become a classic it is appropriate to attempt an explanation of how it best serves the historian. Although Chenu originally wrote the chapters as separate articles, he nonetheless conceives of theology as the all-embracing discipline of the twelfth century and thereby provides an underlying unity to the book. With masterly skill he is able to elucidate the interconnections among speculative thought, literary texts, educational institutions, and social classes. Working with such diverse materials, he searches out their points of contact in an effort to arrive at an interior comprehension of the twelfth century.

Historians will be particularly interested in three of his approaches. The opening chapters probe the internal coherence of certain important concepts of the period, such as nature, man, and the symbolic mentality. An especially helpful study in this line distinguishes the varieties and ambiguities of Platonism in the twelfth century. Chenu then turns his attention to historiography by exploring the mutual interaction between history and theology and the competition among theologians between the historical and the rational approach. The final chapters show how the social context conditioned religious reform. It was not the monks of the rural countryside but the canons and laity of the newly prosperous towns who produced the evangelical awakening of the twelfth century. The populous urban milieu inspired new ideals of preaching and poverty that prepared the way for the mendicant orders. Social conditions were influential not only in popular piety, however, but also in theological speculation. In a noteworthy chapter Chenu traces how the urban environment encouraged the professionalization of the theological master and his perfection of the scholastic techniques of questioning, which in turn transformed theology from Biblical exegesis to rationally

disciplined doctrine. Because of the social acceptance of the professional theological master, the views of theologians could advance beyond those of past authorities. Thus a social environment prompted an element of progress within the traditions of theology. Not only is this a work for theologians, because Chenu understands theology, but also for historians, because of the pivotal place of theology in medieval society. Through Chenu's insights we can better understand the twelfth century.

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WILLIAM ANDERSON. *Castles of Europe: From Charlemagne to the Renaissance*. Foreword by SIR JOHN HACKETT. New York: Random House. 1970. Pp. 304. \$35.00.

This is a strange book, at once difficult to classify and to review. For one thing, the title is completely misleading. Chapter 1 is a survey of the history of fortification from earliest times to the Carolingian era, and the final chapters go far beyond the Renaissance, even up to the "Wimbleton baronial" of late Victorian times. Nor is this volume devoted entirely to castles as the term is generally understood. Fortifications of every variety are dealt with, ranging from the rude enclosures of primitive Germanic and Slavic peoples to the sophisticated mural defenses of Carcassonne and the sixteenth-century outworks of Prague. There are long sections dealing with the castle as the cradle of European secular literature and with medieval strategy and tactics in the open field, topics that are at best peripheral to the main thesis. Even though the page format is generous, to cram this much material, along with 396 plates—many of them full page—and 28 maps and plans, into 293 pages is to do less than justice to the medieval castle.

The reader must also be on the lookout for some fairly obvious misstatements and faulty interpretations. That there were anything like sixty thousand Christian knights at Las Navas de Tolosa in 1212 (p. 151) is open to serious question without supporting evidence that such a number of men and mounts could have been provisioned in a rugged, semiarid terrain that the enemy had occupied for some weeks prior to the Christian advance. It is also fairly well

established that the author of *De Expugnacione Lyxbonensi* was not a cleric (p. 150). A final criticism is that Mr. Anderson has failed to appreciate the professional capabilities of medieval commanders. Following a long-outmoded conception of warfare in the Middle Ages, he insists that the strategic and tactical offensive was the sole expedient of the medieval general. This view ignores completely the successful campaigns of the early Latin kings of Jerusalem, who avoided combat whenever possible and achieved victory by means of strategic maneuver that preserved both the field army and the vital fortresses. Nor is adequate notice taken of the occasions on which the victorious army stood on the defensive to profit from the ill-considered assaults of the enemy, as at Tenchebrai (1106) and the Standard (1138).

Despite the foregoing criticism, which may seem unduly harsh, and despite the formidable price, this is a book that will be indispensable to anyone who has even the slightest interest in either medieval military history or the history of fortification. Nowhere else can there be found a study that brings the military architecture of East Central Europe and the Balkans into the total framework of warfare in the Middle Ages. And certainly in no other volume can be found the extravagant wealth of illustration that the author and his collaborator, Wim Swaan, have contrived to put between covers. If mortgaging the family homestead is necessary, do so; the reward will be well worth the sacrifice.

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J. L. KIRBY. *Henry IV of England*. [Hamden, Conn.:] Archon Books. 1971. Pp. 280. \$7.75.

Those who choose to write biographies of medieval men of affairs realize that their labors must be tilted toward the *res gestae* rather than the *vita*. J. L. Kirby takes care to warn us in advance that his work on Henry IV has "a somewhat old fashioned air" and consists "mainly of the story of his reign."

But if the story is well told a legitimate purpose is still served, for between the long article in the *Dictionary of National Biography* written by T. F. Tout in 1917 and the four volumes of

J. H. Wylie (1884-98) there is little for the reader, either of a scholarly or a popular nature. The historical imagination has always run more readily toward Richard II, Henry's cousin and predecessor or toward Henry V, his son and successor. Shakespeare and the Tudor mythographers were correct in deciding where the drama and the human interest lay.

Kirby has wisely pursued a different line of attack. The subtitle of this book might well be, not "the story of Henry's reign," but rather, "the story of how Henry tried to run the government." Kirby comes to royal biography by way of research on Henry's council, the new and flexible organ by means of which the king hoped to expedite business and load his "cabinet" with his own partisans. Because of this bias we are given an inside view of the eternal problems of government. If not exciting, the story is convincing, written more from the Public Record Office than the Rolls Series. There is also a concise account of the conundrum over Richard II's last parliament—or was it Henry IV's first?—as well as Henry's early travels abroad and his quarrels with his son. Frustration is the keynote of it all. A statement on page 161 sums up not only the year 1403 but all medieval government: "It was true that this year he had survived a great danger, suppressed a revolt and pacified the north. But the position was as difficult as ever."

Every reviewer, even a complimentary one, wants to get a few licks in. Some recent scholarship has been ignored: Richard Jones on Richard II and Thayron Sandquist on the coronation and holy oil, among others. There is very little on what the people of the realm thought about it all: only one reference to a private petition in the Parliament of 1411 touches on the conditions beyond the court. Lastly, some of the excellently chosen illustrations are poorly reproduced. But all things considered, this book is a worthy contribution to the growing body of literature on fifteenth-century England.

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LOUIS TRENARD, under the direction of. *Histoire de Lille. Volume 1, Des origines à l'avènement de Charles Quint.* (Publications de la Faculté

des Lettres et Sciences Humaines de Lille.) Lille: Librairie Giard. [1970.] Pp. xi, 510.

As the first of a projected three volumes on the history of Lille, this book must bear the weight of a *présentation géographique* and several prefaces written by the more or less eminent men who have organized the work of others. The latter include most notably Gérard Sivéry, whose massive sections on socioeconomic and institutional history form the heart of the book, and Henri Platelle, who has written a fine study of the town's religious life. Otherwise admirable sections on origins and on art suffer from a paucity of material; a number of pictures of ruins and reproductions of old views show how much Lille has lost by the destruction of virtually all her medieval architectural patrimony.

Although Lille's site was settled in prehistoric and Gallo-Roman times and the importance of a nearby Carolingian villa is well attested, the town does not emerge into documentation until 1066, when Count Baldwin V of Flanders founded the collegiate church of St. Pierre in Lille's *castrum* (the origins of which are unknown). A *portus* on the river Deûle was also in existence by this time, no doubt benefiting from the *castrum*. Lille subsequently became a major center of cloth manufacturing, her wares and merchants appearing far and wide—most notably in Genoa and then in the fairs of Champagne; her political importance in Flanders is attested by the role she played during the crisis of 1127. She was already, one supposes, in possession of her laws and liberties, but her first surviving charter dates only from 1235, and it is not until the end of the century that her archives become substantial. Her passage into the French royal domain, 1304-69, oriented her commerce to France, and the finer cloth that had been destined for Italy was replaced by a cheaper product as well as by such new products as linens and leather goods—changes reinforced by the English embargo on wool to Flanders in 1336. In her capacity to survive through flexibility Sivéry sees one secret of Lille's success; in a similar sense he notes that the dispersion of manufacturing in the surrounding countryside was a factor preventing the savage guild battles that wrecked more famous Flemish cities. Lille's system of government,

which allowed new groups access to power, also made for inner peace and stability. With inclusion in the Burgundian territorial complex, from 1384 on, Lille rose to the height of her political and cultural importance, partly on the basis of these foundations. (It was not until after the Middle Ages that she again became part of France proper—the Charles Quint of the title was the emperor, not the king of France.)

The authors of this book have served their city well. In a certain sense, as already suggested, the work tends to emphasize Lille's importance, but not unduly and, indeed, always usefully, in the face of general scholarly neglect of Lille in favor of other Flemish towns. The level of discussion is quite high, with coffee-table readability deliberately sacrificed to the sort of socioeconomic thoroughness cultivated by the dominant French school today. On point after point, in consequence, this history of Lille can serve as a case study in a wider context—as Sivéry intends. The question of origins is one such point; another would be the complex interplay of lordships within the city and between the various holders of jurisdictions and privileges: the counts of Flanders, the kings of France, the urban magistrates, the church of St. Pierre (divided into its canons and its provost), the parochial organization, the bishop of Tournai, and the craft guilds. I would have liked a more independent treatment of straight history, rather than the absorption of it into a socioeconomic framework, and I would have been grateful for an appendix of key documents discussed in the text. Otherwise I can only express admiration.

HOWARD KAMINSKY
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KARL BITTMANN. *Ludwig XI. und Karl der Kühne: Die Memoiren des Philippe de Commines als historische Quelle*. Volume 2. Part 1. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, 9/II/1.) Göttingen: Vandenhoeck & Ruprecht. 1970. Pp. 891. DM 90.

PAUL MURRAY KENDALL. *Louis XI: ". . . the Universal Spider . . ."* New York: W. W. Norton and Company. 1971. Pp. 464. \$10.00.

Though widely differing in many important respects these two books are alike in owing their

lifeblood to that celebrated fifteenth-century historical memorialist Philippe de Commines. Professor Kendall's account of the life and character of King Louis XI is largely based on Commines, whom he follows in many an amusingly described incident and perspicuous character sketch, and whose general leanings, in particular the pronounced hero worship of Louis, he reproduces or even exaggerates. Dr. Bittmann, on the other hand, is almost exclusively concerned with exposing Commines's inaccuracies, inconsistencies, and other historical faults. His analysis fully confirms what one has long suspected: that Commines is one of the greatest liars in the history of historical writing. This is unfortunate, perhaps, for writers like Professor Kendall, who choose to use him extensively or tend to inherit his misrepresentations. On the other hand Professor Kendall is a biographer, not a historian, and one ought not perhaps to blame him for rejecting Dr. Bittmann's self-appointed task of trying to sift truth from falsity.

In a sense Kendall's and Bittmann's books represent two extremes of historical writing. Kendall has so little history that one wonders if it is fair to review *Louis XI* in this journal. Notes are nonexistent; the value of new material is nullified because it is used without any documentation; no attempt is made to analyze or sift the available contemporary evidence. On the other hand Bittmann might be accused of abandoning history in favor of pure scholarship. His is an immensely detailed, intricate, and exhaustive analysis of all the available information bearing on every statement of Commines concerning Louis XI and Duke Charles the Bold of Burgundy. Kendall can write. His narrative is vivid, lively, vigorous. He is brief and to the point. He concentrates on creating an image of Louis XI that is at least consistent and competently constructed, even if perhaps as much fictitious as historical. Many of his other images are purely fictitious: the Swiss and their allies, for example, are dubbed "mountaineers," or even "rude mountaineers." Bittmann has fewer literary gifts; he is a scholar. Though he organizes his material well, he tends to be lengthy in his expositions and to drift off into commentary and generalization. But he brings new insights and new information to bear on

nearly every aspect of Franco-Burgundian relations in Louis's reign: part 1 of his second volume, under notice here, discusses these relations in the years 1472-74, together with Louis XI's relations with the Swiss between 1469 and 1475.

Neither of these books provides that detailed, well-documented, scholarly history of France during Louis XI's reign that is so badly needed. Mesmerized by the alleged vagaries of Louis's character and his supposed diplomatic skill and political vision, writers interested in the period have tended to turn to biographies, more or less popular, of the king. Kendall has only done again what was done in 1927 by Pierre Champion. In some respects his book is less interesting and original than D. B. Wyndham Lewis's *King Spider*, published in 1930, which finds no mention in his bibliography. Bittmann, on the other hand, has compiled a massive historiographical work that perforce ignores many of the most important problems of French history under Louis XI. But this work, which is far from complete, will remain an essential tool for serious historians long after Kendall's *Louis XI* has been forgotten and replaced. Meanwhile, the general public will read Kendall and continue to subscribe to the version of Louis XI as a patriotic, crafty, unscrupulous, and brilliantly successful tyrant and probably, ignoring Bittmann as Kendall does (apart from some petulant remarks in an appendix), continue to accept Commynes as a writer striving to be truthful.

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FRANCO SIMONE. *The French Renaissance: Medieval Tradition and Italian Influence in Shaping the Renaissance in France*. Translated from the Italian by H. GASTON HALL. [London:] Macmillan; distrib. by St. Martin's Press, New York. 1969. Pp. 335. \$11.50.

Franco Simone first published this book in Italy in 1961. Treating the period from about 1300 to 1550, he sets out to update our views concerning the origins and direction of the French Renaissance. His strength lies in the analysis of literary influences and the nailing down of traditions. In this study he puts his scholarship at the service of historiography with mixed results.

The following are his main points. Now that the revolt against the "Romantic" views of Michelet and Burckhardt can be more coolly assessed, we have come to appreciate that the French Renaissance was distinctive, although it reflected general and local traditions (introduction). Stimuli provided by the Avignonese papacy helped to rekindle native traditions in the fourteenth and fifteenth centuries; the effects of outside influences, mostly Italian, may thus be seen to have been more modest (ch. 1). From the late fourteenth century French scholars and literati became sorely sensitive to Italian leadership, real or suspected, in the cultivation of eloquence and the revival of classical letters, but here again native differences swiftly emerged: Italian humanists were more addicted to literary style and the delights thereof (or at least were thought to be so), whereas French humanists, following a more traditional track, revealed deeper moral and theological concerns (ch. 2). A strong tradition of scholarship long held that the French fifteenth century exhibited little fresh interest in the pursuit of classical studies. This view is invalid; throughout the course of the century, from Clamanges and Montreuil to Fichet and Gaguin, imposing figures came forth to attest to the vitality of French literary culture (ch. 3). The productive force of this intellectual tradition was vigorously demonstrated in the early sixteenth century, when, in the movement for Church reform, French humanists won the vanguard by arguing that the new weapon of the *miles Christi* had to be learning (ch. 4). The native thrust of French letters was also manifested in the reception accorded to Petrarch's writings even in the sixteenth century: the French emphasis was unmistakably moral (ch. 5). Finally, an appended review-discussion of Paul Renucci's *L'Aventure de l'Humanisme européen au moyen âge* (1953) is used to draw attention to the scholarly preparation of the best of the fourteenth-century humanists, the better to highlight the distinctive nature of the Renaissance, in contrast to the renaissances of the twelfth or earlier centuries.

Some of Simone's points are certainly worth making, particularly those of the second and fourth chapters. But his arguments are not well served by a plaintive tone, an insufficiency of

evidence with respect to the claims, and a mediocre translation.

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WILLIAM M. BOWSKY. *The Finance of the Commune of Siena, 1287-1355*. New York: Oxford University Press. 1970. Pp. xix, 379, 3 maps. \$16.00.

The history of public finance in late medieval and Renaissance Italy remains largely unwritten. William M. Bowsky's new book is therefore a welcome beginning and should serve to stimulate further work in a neglected field, indeed, a field that historians who generalize about government, society, economy, and civic and religious ideology ignore at their peril.

Bowsky adheres rigorously to the plan he has set himself: to analyze the machinery of Siena's fiscal administration and the variety of governmental expenses and sources of revenue during the *buon governo* of the nine governors and defenders of the Commune of Siena. He lists numerous minor taxes, fines, and fees; describes the *dazio* (direct tax) and the many gabelles in detail; assesses the significance of forced and voluntary loans and the "pawning" of public properties; and evaluates the tax burden borne by the *contado*. He devotes less space to expenses in these years, but concludes that Siena, like most city-states, had a rapidly expanding budget, a good part of which was devoted to military expenditure. He ends with a chapter comparing Siennese public finance to that of other communes about which something has been written. The text is followed by fourteen appendixes of varying utility, in which documents are reproduced and chronologies compiled, and three fold-out maps, clearly intended to be no more than broadly illustrative, since two are dated 1599 and the third 1723. There is no bibliography.

Bowsky has brought together an impressive array of data, almost wholly from the archives, which he knows and uses well. His descriptions and conclusions will doubtless prove valuable guidelines for students of other Italian communes. But out of his careful factual analysis tantalizing questions arise. Most obvious are those involving the civic values of the Siennese—

the culture, if you will, of public finance. Other historians have noted a strong statist tendency in Florentine fiscal practices and institutions in the course of the fourteenth century, accompanied by a major transformation of the citizens' attitudes toward their government and their role as political men. Bowsky withholds comment on what happened at Siena. He also withholds comment on such interesting questions as why Siennese authorities, as quoted in a tax order of 1324, believed the *contado* communities and the *contadini* should be treated "benignly" (*benigne tractare*) for tax purposes. Were the reasons political, economic, humanitarian, or ideological? We don't know. But perhaps I expect too much of the first modern book of quality on Siena in the late Middle Ages. A second book, which I hope will be of wider scope, is in preparation.

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BAYARD DODGE, editor and translator. *The Fihrist of al-Nadīm: A Tenth-Century Survey of Muslim Culture*. In two volumes. (Records of Civilization: Sources and Studies, Number 83.) New York: Columbia University Press. 1970. Pp. xxxiv, 570; 572-1149. \$40.00 the set.

In A.D. 987 or 988 the Baghdad bookseller Abu al-Faraj Muhammad al-Nadīm completed his *Fihrist* or catalog. It may have been begun as simply that, a catalog for the patrons of his father's and his own bookstore. But as his own reading and interests grew the catalog was transformed into a guide and, in the end, into an encyclopedia not merely of the contents of one tenth-century Baghdad bookstore or, indeed, of the wide-ranging literary interests of one man, but of an entire civilization.

"This is a catalogue of the books of all peoples, Arab and foreign, existing in the language of the Arabs, as well as their scripts, dealing with various sciences, with accounts of those who composed them and the categories of their authors, together with their relationships and records of their times of birth, lengths of life, and times of death, and also of the localities of their cities, their virtues and faults, from the beginnings of the formation of each science to this our own time, which is the year three hundred and seventy-seven after the Hijrah."

With that as prelude the *Fihrist* unfolds: language and calligraphy; sacred scriptures, including the *Qur'an*; grammarians; historians and learned civil servants; courtiers and jesters; theologians and their sects; the great jurists of Islam; the Greek sciences of philosophy, mathematics, astronomy, and medicine; a marvelously entertaining collection of storytellers on every subject under the sun, including one of the very earliest accounts of the *Thousand and One Nights*; treatises on the history of various non-Islamic sects; and a final book on alchemy and alchemists. The whole is richly interspersed with the author's own historical and critical comments.

The Western tradition knows no other book to compare with this until the introduction of printing. The *Fihrist* is peculiarly the product of the sophisticated skills and interests of the tenth-century capital of Islam, just as its only distant rival, the *Bibliotheca* of Photius, reflects those of Constantinople a century earlier. The contrasts are instructive. The *Bibliotheca* is an anthology of excerpts of one man's reading; the *Fihrist* is an equally personal document, but it is an ecumenical book exactly to the extent that Islam was an ecumenical civilization. Al-Nadim was instructed, in a way that Photius could never hope to be, on the high literatures not merely of the Greeks and Arabs but of Iran, India, and China as well.

Al-Nadim's tastes were as happily catholic as his historical perspectives. Though a Shi'ite by conviction, and likely a Mu'tazilite as well, he interested himself in the entire range of Islamic and foreign sciences. What we know of the early history of the Muslim theological sects and of such non-Muslim groups as the Sabians of Harran, the Manichaeans, and the Daysanites comes immediately and indispensably from the *Fihrist*. And it is to the same source that we owe most of our information on the progress of Hellenism in Islam. Across the dense pages of the *Fihrist* it is possible to trace, with a convincing amount of detail, the enormous translation activity from Greek, Syriac, and Pahlavi into Arabic.

By the time that al-Nadim wrote, the great translation movement that swept Plato, Aristotle, Euclid, Ptolemy, Galen, and others into an accurate and elegant Arabic was all but over. His friends and associates were closely connected with that movement, and as a result

al-Nadim's information, set out in organized and rich detail in book 7 of the *Fihrist*, is of excellent quality. He knew, for example, the translation history, both Syriac and Arabic, of every school treatise in the Aristotelian corpus, as well as that of many of the Greek commentaries on those works that were being read and studied in bookstores like his own.

There are dark corners, too, in al-Nadim's portrait of Hellenism in Islam. For all his learning al-Nadim had no place for Aeschylus in those pages, nor for Thucydides, and Homer was only a faint wraith for the Muslim. Islam's Hellenism was selective and incomplete, as was late antiquity's own. The *enkyklios paideia* never came into Islam, and the chapters where it might have shone forth are given over instead to the late Hellenes' new preoccupation, alchemy and the occult sciences.

Al-Nadim was not unique in Islam. Many of his contemporaries had something approaching his own range of learning and interests, and there were libraries even in provincial capitals like Nishapur and Bukhara that could rival Baghdad's own. But perhaps only a genuine bibliophile could have written something as genially instructive as the *Fihrist*. And only a genuine humanist could have translated it.

Bayard Dodge, the president emeritus of the American University of Beirut, chose the seventh and eighth decades of a strenuous and distinguished career to tackle this formidable task. Just how formidable is apparent from the outset. The edition of the *Fihrist* still in current use is that done by Gustav Flügel in 1871. A new edition of the Arabic text has been promised for nearly forty years, and Arabists, who necessarily lean heavily on this unparalleled source, have long been aware that important new and more complete manuscripts are available. Dodge's translation is based on those manuscripts, and he served, as the title page justly indicates, as editor before he could serve as translator.

Many of the *Fihrist*'s citations are of names and titles transliterated from a variety of foreign languages. The chore of determining or even deciphering some of the manuscript readings must have been difficult indeed, but it probably accounted for only a fraction of the translator's apparently inexhaustible energies. Dodge has also undertaken to identify all the

authors cited by al-Nadim and to give some explication of the text itself in footnotes, a glossary, and a two-hundred-page biographical index at the end.

Some of Dodge's preciously collected information will doubtless be corrected; I had my own hesitations on some few of the identifications. The publisher should certainly have got the Arabic design right side up on the cover, and it would have been convenient to have Flügel's page numbers printed on the margins. I say these things only because I must. I believe that every scholar or student who interests himself to any degree in the intellectual history of the West has contracted an immense debt to Abu al-Faraj Muhammad al-Nadim and to his first and only translator, Bayard Dodge.

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MODERN EUROPE

GERHARD OESTREICH. *Geist und Gestalt des Frühmodernen Staates: Ausgewählte Aufsätze*. Berlin: Duncker & Humblot. 1969. Pp. 355. DM 48.

This collection has a value that goes beyond the obvious convenience of providing easy access to important but scattered articles of Germany's outstanding historian of early modern Europe. What this value is lies somewhere between the integral theme promised by the titular "spirit and structure of the early modern state" and the range of themes inferable from the subtitled "selected essays" published over a span of thirty-four years (1935-69).

Certainly the ensemble reveals a coverage and a coherence that were obscured by the separate occasions and the specific subjects of the original articles. There are, indeed, threads of a purely formal unity that seem to integrate the whole volume: each of the two sets into which the articles are classified, "European Development" and "German Empire and Territories," contains topics that relate it to the other (neo-Stoicism in Europe and Prussia; the German constitution and the European state system). The variable relationship between centralized authorities and corporate agencies that defines the early modern state as a historical category, moreover, furnishes the common criteria for the selection and interpretation of

both European and German materials. From this formal point of view the volume as a whole can be seen as the verification, by reference to Germany, of the spatial and temporal variety asserted in general of the European early modern state and as the demonstration of the process that rooted this type of state in the principalities rather than in the empire—a unified view of the work that misses its originality and its substantive contributions.

For in fact the volume contains two separate books, and its value consists in what distinguishes rather than in what connects them. The essays in the European section are intellectual analyses that present a challenging interpretation; the essays in the German section are administrative and constitutional surveys that present a comprehensive description. The European section focuses on a new primary idea among the principles of the modern state; the German section organizes the modalities of the early modern state into a complete and evenly diffused spectrum of bureaucratic-corporate relations.

The more provocative of these books by far is the group of essays on the European level, for here Oestreich undertakes nothing less than a reinterpretation of the two most familiar processes in early modern history, secularization and state-making, and he undertakes, moreover, to explain these transitions by a common factor. Five of the essays in this section are concerned with the movement of neo-Stoicism, with the ideas and career of its central figure, the Dutch humanist Justus Lipsius, and with its influence, and Oestreich argues that its moderation, its practicality, its eclectic blend of new-fangled realism and traditional virtue, its stress on the continuity of moral and political discipline—all made it not simply *a* but *the* chief strand leading from the religious and corporate pre-occupations of the sixteenth century to the political and military modernity of the seventeenth. Oestreich counts enough editions and unearths enough acknowledgments to make an impressive empirical case for his thesis, but what is perhaps more persuasive is his shrewd conversion of neo-Stoicism into a historiographical philosopher's stone. In his hands the neo-Stoic movement, as a practical rather than a theoretical doctrine, escapes both the logical and the social problems of a rigorous, elitist

system and becomes an attitude that, despite or because of its inconsistencies, becomes the active spirit pervading the real policies of bureaucrats, officers, and corporate representatives. Oestreich's neo-Stoicism thus inhabits a magical middle ground: it produces the kinds of ideas of sovereign authority and civic loyalty that practitioners actually used, and it equipped men with a spirit of discipline that permeated royal and corporate agencies, mollifying their institutional opposition. Because it is a flexible attitude that permeates all kinds of structures, Oestreich's neo-Stoicism is a most appropriate theme for such a series of essays: it operates equally well on a synthetic and a specialized level, on the genesis of European absolutism and on particular Orangist military reforms, making the same general point for both. The point is that the characteristic forms of the modern state, like the characteristic orientation toward the secular life in general, were sponsored by a more authentic ethical drive than their overt emphasis on power and welfare usually indicates. Oestreich concentrates in this volume on the part of this process that produced sovereign authority and social discipline, both military and civil. Only one essay, on the connection between the religious idea of the covenant and the secular doctrine of the political contract, touches on the parallel process that eventuated in the modern doctrine of human rights, a process that Oestreich himself has treated elsewhere (*Geschichte der Menschenrechte und Grundfreiheiten im Umriss* [1968]).

But this collection of European essays does show some defects in its virtues. If the diversity of the specific subjects reinforces the impression of the ubiquity of the neo-Stoicism that is common to them, it also entails the inevitable repetitions of points that must be communicated anew to the new audiences of each piece. Again, if the looseness and malleability of neo-Stoicism as a practical doctrine obviates the necessity of a rigorous internal analysis, it does raise problems as to where it leaves off and other currents of ideas begin. There is, for example, an inadequate distinction in Oestreich between neo-Stoicism and either original Stoicism or the stoicism already embodied in Christian tradition, and he occasionally takes as evidence of specific neo-Stoic influence references to con-

stancy or self-discipline that could easily come from other inspiration.

No such problems infect the collection of German essays. The four general articles on the imperial constitution and on the developing relations of princes, estates, and armies within the territorial states are the most informative epitomes I know of German constitutional and political history in the early modern period. Oestreich takes off from Hintze, but he encompasses the German principalities in a genuinely comparative view that is free from the Prussocentrism of his illustrious predecessor. Even Oestreich's early (1935) monographic essay on princely "personal government"—that is, government from his own "chamber" rather than through the court council—in the sixteenth century has this broad canvas.

Only the final article, on "Graf Johannis VII. Verteidigungsbuch für Nassau-Dillenburg 1595," seems picayune, but the appearance is deceptive. At its start Oestreich refers to the Dutch army reform instituted by the Nassau-related House of Orange and discussed in his first European essay, and in its conclusion he draws a comparison, contrasting the aggressive and organized realism of the European-oriented Dutch with the confession- and militia-minded defensiveness of the German, which is the one explicit statement of the implicit theme connecting the two collections.

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M. M. SMIRIN. *K istorii rannego kapitalizma v germanskikh zemliakh (XV–XVI vv.)* [On the History of Early Capitalism in German Lands (15th–16th Centuries)]. (Akademiiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka." 1969. Pp. 405.

Western students of the late Middle Ages are already acquainted with the scholarship of M. M. Smirin through his Russian study on Thomas Müntzer (1955), which has also been made available in German translation (1956). The present study adds considerably to our information on the development and administration of the mining industry in Central Europe during the late fifteenth and early sixteenth centuries. In five chapters the author surveys the financing of silver and copper mines, mainly

in Saxony and the Tyrol, and traces the roots of the social and religious conflicts of the times in the economic conditions of the rural mining countryside.

The author presents a well-researched and profusely documented study. His use of sources and secondary literature reveals a profound familiarity with the economic and social history of the Holy Roman Empire. As is frequently the case when solid research is combined with Marxist interpretation, however, revealing observations on details are marred with superficial or unfounded generalizations. There is no objective evidence that the princely authority in the Tyrol or in Saxony was the result of certain specific "Germanic conditions" (*germanskie usloviia*); nor is there any basis for considering the social unrest among the Tyroleans and Saxons to be a manifestation of progressive forces of a "German nation" (*nemetskii narod*). The Tyroleans and Saxons did not, and do not, constitute parts of a single German nation.

In some places the author has overestimated the role of private enterprise to the neglect of the limits posed by the interests of the state. Although the large sums necessary for investment in the mining industry were supplied by the private sector of the economy, mining was a part of the regalia, and therefore ultimately controlled by the "state"—that is, the princely administration—for its own benefit. In addition, while stressing the elements of social conflict the author necessarily had to omit the positive social concern of the "capitalists." In this connection one must note the absence of any reference to the charitable foundations of the Fugger family.

For the critical reader this study will, nevertheless, be a positive Marxist contribution to the understanding of the economic and social changes in the late Middle Ages. Scholars in the West would welcome a translation of Smirin's new book.

IMRE BOBA

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J. P. COOPER, editor. *The New Cambridge Modern History*. Volume 4, *The Decline of Spain and the Thirty Years War, 1609–48/59*. New York: Cambridge University Press. 1970. Pp. xxi, 831. \$12.50.

With this volume *The New Cambridge Modern History* bridges that difficult gap—the early seventeenth century—that always seems to afflict series histories; in virtually every one, the volumes covering this period are the last to appear, and for good reason. The long parenthesis between Philip II and Louis XIV, the two generations of conflict that substituted French for Spanish hegemony and shook the foundations of every European state—this is a complex and thorny period that historians have preferred to skirt in their progress from Renaissance to Enlightenment. Anomalies are always best ignored. But over the last decade or so, attention has focused on "the crisis of the seventeenth century," and the period has finally become fashionable as contemporary parallels suggest themselves; a new view is emerging of the period and its place in the general development of Europe. The long gestation of this volume is thus easily understandable. The need for a new synthesis is by now clear and pressing; this work in many ways answers the need, even though it is perhaps still too early for the full revaluation that is required, for the dust of controversy still hangs heavy in the air. In his introduction the editor attempts—courageously and with reasonable success—to summarize and reduce to order the recent debates. This is the only point at which the theme of "the crisis of the seventeenth century" is explicitly treated; but it is implicit throughout.

The selection of chapters provides a generally satisfactory coverage, although there should perhaps have been a separate account of the papacy during the critical period from the triumphs of the Counter-Reformation to the humiliations of the Peace of Westphalia. (This deficiency is not made good by the chapter on "Religious Thought," which is better on Protestantism.) The central chapters dealing with Western Europe form a solid armature around which the rest of the work takes shape, although the emphasis is not on Western Europe alone. The excellent chapters on Eastern Europe, especially those on Poland and the Ottoman Empire, remind the reader that the symptoms of crisis were general and widespread: Murad IV and Michael Romanov faced much the same problems as Richelieu and Olivares. The wider picture is completed by a useful series of chap-

ters dealing with Europe overseas. But the conflicts of the West European states, internal and external, form the core of the book. The chapters on "The Spanish Peninsula," on "French Institutions and Society," on "The Low Countries," and on "Sweden and the Baltic" are of a uniformly high order, while that on "The Scientific Movement" provides lucid, balanced, and informative treatment of an essential theme. One may contest some of the judgments; Professor Roberts, for instance, seems to underestimate the predatory character of Swedish expansion (pp. 393, 398), while Professor Mousnier's account of peasant revolts (pp. 492-94) ascribes too much influence to the machinations of the privileged orders. Professor Trevor-Roper contributes a stimulating chapter on "Spain and Europe, 1598-1621" in which he extends his familiar court-country polarization to embrace the whole of Europe, with Spain and its hangers-on in the role of the "court." This has the disadvantage of making the burghers of Amsterdam the leaders of the "country," which alters the original terms of the argument, but nonetheless provides a guiding thread through the maze of international politics in the uneasy peace before the Thirty Years' War. In his chapter on "International Relations, 1648-60" Professor Livet is less successful in treating an equally difficult period, but the very complexity and compression of his chapter reveal the problems involved in dealing with a period that is still inadequately known—historians' interest seems to flag after Westphalia—yet that forms the culminating phase in the destruction of Habsburg power by France.

The overall standard of the volume is competent and workmanlike, with one or two exceptions, among them the editor's chapter on "The Fall of the Stuart Monarchy." After his asides at the expense of "l'histoire totale" (p. 10) and socioeconomic interpretations of the English Revolution (pp. 542-43), we are entitled to expect more from him than he in fact provides. He reduces the greatest drama of the period to a sequence of parliamentary and administrative maneuvers that ultimately fail to explain why a war had to be fought and a king executed. The wood is completely obscured by the trees. Professor Lough's chapter on "Drama and Society," while offering some

useful information on the political and propaganda uses of the theater, finally contradicts itself by dismissing as nonsense (p. 258) any attempt to regard Richelieu's support for the classical rules of drama as "another aspect of his desire to fasten an iron control over the whole life of France"—the view that is in fact maintained by Professor Mousnier on page 491. And if we are to have a chapter on the theater, why not another on the equally important developments in art and architecture? Bernini, Rubens, Velasquez, and Rembrandt merit more than the fleeting acknowledgment they receive. Finally, some mention must be made—churlishly perhaps—of the considerable number of misprints and failures to abide by standardized forms of foreign names. This type of work is intended primarily for students, and mistakes like this can be misleading to the unwary or uninstructed reader. The value of the work is lessened by the failure to identify Regensburg (p. 343) as being the same place as Ratisbon (p. 344), or to make immediately clear that Albert of Valdstejn (p. 519) is merely our old friend Wallenstein under a Czech alias. Citation of foreign titles is also rather slapdash: a quick check turned up ten errors in the footnotes to the introduction. And to spell Nuremberg three different ways seems excessive. A more serious mistake on page 262 reverses the terms of the Treaty of Lyon: France abandoned and did not receive Saluzzo, "the gateway to Italy." But these are minor points. This volume offers a generally competent synthesis of a troubled and baffling period, and it has moments of excellence. Volume 12 of the series has already been revised, and this volume too may well need revision in a few years. This is not a criticism, but a necessary observation on the rapidity with which views of the early seventeenth century are changing. Any synthesis is provisional, and this one is bound to be even more so than most.

GEOFFREY SYMCOX
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Los Angeles

P. G. ROGERS. *The Dutch in the Medway*. New York: Oxford University Press. 1970. Pp. xiii, 192. \$7.25.

In June of 1667 a Dutch fleet captured the English fort at Sheerness, broke through a

defensive chain near Gillingham, and moved as far up the Medway as Upnor Castle before meeting serious resistance. There the fleet lost about fifty men, and the decision, perhaps an overcautious one, was taken to terminate the operation without making an attempt on the Chatham Dockyards. Even so the Dutch reaped glory from their investment of fifty men and nine or ten fireships, for they took home with them the *Royal Charles* and the *Unity* as proof that at this time there was only one maritime power. A month later England was obliged to make a humiliating peace at Breda.

The Dutch call it the Chatham expedition, but in those terms the operation was a failure since the fleet did not reach the Dockyards. The English, with more justice, refer to the Medway disaster. The precise terms of the Treaty of Breda were not too unfavorable to England. If she lost Surinam and Pularoon she gained New York and New Jersey. She was, of course, obliged to modify the Navigation Acts in favor of the Dutch. But the real disaster was to the prestige of the restored monarchy. Oliver had been dead for less than nine years, and the glories of his day were still remembered. To the capture of Jamaica and Dunkirk the disaster on the Medway posed a dreary contrast. We should perhaps look on the War of 1665 not as a commercial struggle but as a constitutional one between opposing republican and monarchical principles, a struggle in which the republicans gained a clear victory.

Mr. Rogers, an amateur of history, writes well and knows his Dutch. He gives us a clear and straightforward narrative, even if it is less exciting than the one in Pepys. Although *The Dutch in the Medway* will be of no interest to scholars, it may perhaps bring students into their classrooms.

STEPHEN B. BAXTER
University of North Carolina,
Chapel Hill

GUILLAUME DE BERTIER DE SAUVIGNY, *Metternich et la France après le Congrès de Vienne*. Volume 2, *Les grands Congrès—1820/1824*. Bibliothèque des recherches historiques et littéraires.) [Paris:] Hachette. 1970. Pp. 280–914.

This book, the second of a three-volume work, shows once again the thorough archival research, careful exposition, sound insight, and

serene Rankean objectivity typical of Professor Bertier's scholarship. Without repeating or sharply revising the general diplomatic story of 1820–24, Bertier nonetheless tactfully corrects existing accounts (including mine) at many points and supplies a detailed exposition of the Austro-French competition for European leadership containing many interesting nuances. We learn, for example, that France was almost as fearful of British designs on Sicily as of Austrian designs on Naples and that Richelieu opposed Austrian armed intervention partly because he genuinely feared it would fail, encouraging general revolution. Bertier demonstrates that the interminable rivalry over Italy lay behind all the disputes over Spain. He shows both sides restrained in their contest by their common antirevolutionary ideology. France surprisingly more than Austria in certain instances. All the dramatis personae are portrayed in penetrating and interesting fashion. This is certainly a definitive work.

Yet there are possible criticisms. The book is too long. True, the subject matter is complex and the treatment necessarily detailed, but some rather trivial questions are given more space than they deserve. Moreover, the author's practice of publishing long excerpts from the diplomatic exchanges means that page after page is covered with extensive quotations in fine print, many of which could have been greatly compressed or entirely omitted. Few readers will plow through all of these extracts; they often serve to weigh down the narrative and obscure the detective work Bertier has done.

In addition the author's conclusions, while judicious, seem a bit narrowly confined to his immediate subject and time period. Bertier appears to assume the legitimacy of the aims of France and Austria, assessing merely their success or failure in fulfilling them. Are not the goals themselves subject to question? Was not the real question for France, as Pasquier and Richelieu debated it, whether her traditional anti-Habsburg policy was still viable—whether pursuing national independence and glory above all else was not already anachronistic? Perhaps the strongest criticism of Metternich is not that he tried to curb France's independence—he may have been right in considering that any independent France would be ambitious, and any ambitious France revolu-

tionary—but that he did not try to curb her independence by sharing Italian hegemony with her, as he and other Austrians would later try to do. Again, I think Richelieu and others showed insights into British policy that most historians have ignored and that Bertier might have emphasized.

To be sure, these are questions of style and interpretation; it is churlish to complain of omissions when so much of value is offered. Yet I suspect that the worth of this masterly study would be more apparent, especially to readers skeptical of traditional diplomatic history, if its lessons and insights were applied more clearly to the wider story of the nineteenth century. Perhaps the concluding volume will do this.

PAUL W. SCHROEDER
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Urbana-Champaign

H. R. KEDWARD. *Fascism in Western Europe 1900–45*. New York: New York University Press. 1971. Pp. xi, 260. \$6.95.

The prime intent of Mr. Kedward, lecturer in European history at the University of Sussex, is to offer a definition of the concept of fascism; his narrative of the rise and fall of several fascist movements is no more than a sketch to illuminate general propositions. His book is thus the obverse of F. L. Carsten's *The Rise of Fascism* (1967), which mostly steered clear of phenomenological explanation and left the facts to speak for themselves. Kedward's prospective audience is a student one, British sixth-formers and Anglo-American undergraduates—a generation much addicted to applying the epithet "fascist" abusively while understanding it little historically.

Kedward holds that the essence of fascism is to be found in a peculiar blend of contradictory elements from the nineteenth century—capitalism and socialism, the progressive and the primitive, elitism and mass politics. This synthesis is elaborated in terms of psychological, social and economic, and cultural approaches to fascism. The notion of a synthesis is useful, if only because it intimates plainly that fascism cannot be simplistically assigned to either a right-wing or a left-wing category.

On the other hand, there are some disturbing lacunae in this book. The revulsion against the

atomization of modern society and the longing for a new social cohesion—surely the starting point of every fascist movement—is implied rather than stated. Nor is there any systematic appraisal of the catalytic, brutalizing effect of the First World War; without this, the advance of fascism is scarcely credible. But by far the most serious flaw concerns the encompassing of Hitler and Mussolini within one interpretation of fascism. So great were the differences in kind, not just degree, between the clerico-corporative tradition spawned by Mussolini and nazism's *völkisch* racism that some commentators (G. L. Mosse, for example) have concluded that the two phenomena were distinct. Kedward, reasonably enough, prefers to place Nazi Germany in the context of Western European fascism; but in doing so, he never really comes to grips with the immanent dichotomy between the Mussolinian and Hitlerian experiments. Significantly, Austria, where the two styles of fascism contended face to face for the soul of the nation from 1930 to 1938, is not discussed at all. Like Ernst Nolte and John Weiss among recent writers on fascism, Kedward, too, might be accused of forcing his subject to fit a procrustean definition.

In sum, one can commend *Fascism in Western Europe* as a readable introduction. But also the beginning student should be warned that the book is far from comprehensive and that he needs to sample other authorities.

ALAN CASSELS
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JAMES E. MCSHERRY. *Stalin, Hitler, and Europe. Volume 2, The Imbalance of Power, 1939–1941*. Cleveland: World Publishing Company. 1970. Pp. viii, 357. \$12.50.

This volume, which continues Mr. McSherry's earlier study, begins with the establishment of a new order in Eastern Europe resulting from the collaboration of Nazi Germany and Soviet Russia reflected in the treaties of August 23 and September 28, 1939. The volume ends with Hitler's attack on Russia in June 1941, which destroyed this new order.

Mr. McSherry presents a clear picture of diplomatic moves and countermoves in Europe between September 1939 and June 1941, and he focuses attention on Soviet expansion into the Baltic States, Finland, and Southeast Eu-

rope. He makes good use of the familiar sources, especially the two indispensable compilations of diplomatic papers, *Foreign Relations of the United States* and *Documents on German Foreign Policy*. Generally speaking, however, Mr. McSherry is telling a story that is pretty well known. By making Stalin rather than Hitler the central character in the picture the author has tried to change the focus of the observer who has been accustomed to viewing this period of history in the light of Hitler's actions rather than of Stalin's reactions. Yet it is doubtful that this approach enhances our understanding of the events.

Mr. McSherry's analysis of Hitler's decision to attack Russia does not deal quite sufficiently with the evolution of the Führer's grand strategy that produced that decision. Hitler hesitated between short-range and long-range objectives and considered for several months the conflicting alternatives of an anti-British continental coalition with Soviet participation and a continental empire built on the ruins of the Soviet Union. The most authoritative study of this subject is still Andreas Hillgruber's masterly work *Hitlers Strategie: Politik und Kriegsführung, 1940-1941* (1965), which Mr. McSherry fails to cite either in his bibliography or in his footnotes.

In a final summary of the results of two years of Nazi-Soviet collaboration the author engages in speculation on possible alternative courses that the Soviet Union might have adopted in 1939. He also castigates Soviet policy makers for being unable, for ideological reasons, to perceive essential differences between various capitalist states and for wanting to win "everything or nothing" in the international arena. In the light of Soviet and Communist policies since 1917, this seems to be a questionable generalization.

ARTHUR G. KOGAN
Department of State

Le Saint Siège et la guerre mondiale, juillet 1941-octobre 1942. (Secrétairerie d'État de Sa Sainteté, Actes et documents du Saint Siège relatifs à la seconde guerre mondiale, Number 5.) Vatican City: Libreria Editrice Vaticana. 1969. Pp. xxvi, 794.

The Holy See was determined to maintain its neutrality throughout the war, a condition

deemed essential to the discharge of its religious and moral mission. The effort was lonely, often misinterpreted, and, in the period covered here, unsupported by any great power. Neutrality imposed the need for difficult choices. Of this series of documents, volume 2, *Lettres de Pie XII aux Evêques Allemands, 1939-1944* and volume 3, *Le Saint Siège et la situation religieuse en Pologne et dans les Pays Baltes, 1939-1945*, examining special and controversial issues, revealed how agonizing were the wartime decisions made by the pope and his secretariate as the Germans established themselves as conquerors of Central Europe. The present volume contains 511 documents, arranged chronologically, that show in other cases the determination of the Vatican to avoid alignment. The issues dealt with here are of lesser magnitude than those recorded earlier. To the papacy still came intimations of plans for peace, but gone are efforts to seek out the basis for a negotiated settlement, as seen in volume 1, *Le Saint Siège et la guerre en Europe, 1939-1940*. There is a general recognition of the neutralist position asserted in volume 4, *Le Saint Siège et la guerre en Europe, Juin 1940-Juin 1941*. In the volume under review the Vatican responds to issues arising from the expansion of the conflict into global war, the invasion of the Soviet Union, and the entry of the United States into the conflict.

To the antagonists in these great events the Vatican was of only marginal concern. What interest there was was directed at testing to see if the papacy could be swayed. We are given the Vatican record of German and Italian efforts to enlist the papacy in an anti-Bolshevik "crusade." Publicly opposed to the Communist regime, the pope would give no declaration in favor of a war against the Soviet Union launched by Nazis who were themselves under Vatican attack for their persecutions of the Church. "Un diavolo caccia l'altro," Tardini told the Italian ambassador who was arguing on Germany's behalf. This volume shows the Vatican's perspective on the various subjects raised by Myron Taylor's missions, the second of which, in September 1942 in the face of soundings for a compromise settlement from German sources, presented the American goal for a war to total victory. Taylor firmly rejected the possibility of any diplomatic settlement: "We have deter-

mined that we cannot deal with faithless men." There are some interesting observations on American policy here.

Other matters presented in this volume include the safety of Vatican City in case of Allied bombing raids against Rome, concern with the Church in Latin America, and the request of the Japanese government after Pearl Harbor to establish diplomatic relations with the Holy See. Despite protests from the United States and China this was agreed to. A dispatch to the apostolic delegate in Washington, carefully edited by the pope, was sent to allay Roosevelt's concern. It stated, in terms of "absolute impartiality," the Holy See's sole and strictly nonpolitical desire to protect Catholic charitable interests and reflects the themes of this volume, neutrality, moral purpose, and the protection of the Church's institutional position worldwide. The documents are in their original languages, and the editing was done in French. There is no analytical table, but each document receives a line or two indicating its main theme, there is a good index, and the editors open the volume with a useful sixty-page introduction.

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ELIZABETH FRANCES ROGERS, editor. *The Letters of Sir John Hackett, 1526-1534*. (Archives of British History and Culture, Volumes 1-2. Published by the Conference on British Studies at West Virginia University.) Morgantown: West Virginia University Library. 1971. Pp. xxiii, 419.

The convenience of printed historical sources is so great that to question a work such as this may seem churlish. But neither historians' time nor publication funds are inexhaustible, and questions of relative utility must be asked. In the case of this collection of the letters of the English ambassador in the Low Countries from 1526 to 1534, this reviewer is compelled to come to a negative judgment. The fundamental problem is that Hackett, for all the dignity of his title, was more a source of news and an exalted consul than the king's alter ego over the water. Men who first made their mark in trade, like Hackett, however useful in questions of trade and monetary exchange, were not entrusted with the responsibility of high policy. Hackett wrote

with accurate modesty of his own "lytyll ondyr standyng" (p. 221), and Stephen Vaughan confirmed that he was "a discrete gentleman but is no man profoundly to reason a matter of great weight" (p. 306). There are useful things in these papers, but virtually all easily available to the historian, especially in the *Letters and Papers* of the reign of Henry VIII. True, the pieces calendared there are here given in full and meticulous transcription, but without visible advantage to the scholar not enamored of the vagaries of early sixteenth-century English orthography for their own sake. The annotation is moderately elaborate and yet surprisingly limited: most of the explanatory notes are simply summaries from the *Letters and Papers* and often give information not to the point of the document at hand; while the contributions of generations of scholars in England, France, and the Low Countries are almost completely neglected. Most identifications are obviously correct, but the bales of alum landed at "able Dermude" in Zeeland were probably put ashore at Arnemuiden. The Dutch *schout* was the same official as the English sheriff; if the *Shorter Oxford Dictionary* does not give "scoutet," how many readers will be helped by this translation? Finally, the introduction is far too brief to be truly useful.

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R. B. SMITH. *Land and Politics in the England of Henry VIII: The West Riding of Yorkshire: 1530-46*. New York: Oxford University Press. 1970. Pp. xviii, 318. \$11.25.

A three-hundred-page analysis of what amounts to a single county during fifteen years is a very detailed study. Experts cannot be disappointed by Dr. Smith's revised dissertation, since a careful and imaginative discussion of virtually all surviving evidence is definitive. Yet the book has a wider appeal. Among the most important developments in Henry VIII's reign were the Pilgrimage of Grace and the confiscation and later alienation of the monastic estates. The West Riding was the most significant area in that most dangerous of Tudor rebellions, and in it lay some of the richest of monastic properties.

In his introduction Dr. Smith explains how he builds upon the work of Tawney and other participants in the controversy over the gentry. "To relate economics to institutions and politics means more than casually linking the economic history of small localities to the political history of the nation. The same people, whether in London or in some remote corner of the realm, must be studied simultaneously from an economic and a political point of view."

Very valuable are two appendixes. Appendix 1 elucidates the technicalities of many sorts of records; this information can usually be learned only in a graduate seminar or in the hard school of self-education. Appendix 4 lists nearly seventy-five principal gentry together with their incomes and their assessments for the subsidy.

In the six chapters that constitute the body of the book the author analyzes in depth the economic and social structure of the West Riding, the personnel of local government—"Men of Power and Worship"—the course of the Pilgrimage of Grace, and the fate of the monastic lands between 1536 and 1546.

Two passages summarize Dr. Smith's splendid work. "What happened between the thirteenth and the seventeenth century was that they [the gentry] became emancipated from the 'medieval' system of lordship over land; within the 'modern' system, as property-owners, they became the equals of the nobility before the law, differentiated from them only by the fact that most noblemen had larger estates and more revenues than most gentry." "The Pilgrimage of Grace failed because the 'feudal' power of overmighty subjects was no longer an adequate basis for successful rebellion; and because devotion to a principle was not in itself sufficient basis for a movement powerful enough to defeat the Crown. . . . That was the true significance of the 'rise of the gentry' in the sixteenth and seventeenth centuries."

JOHN H. GLEASON
Pomona College

MARY M. LUKE. *A Crown for Elizabeth*. New York: Coward-McCann. 1970. Pp. 573. \$10.00.

Mrs. Luke lives (we are told) with her daughter Melinda in Ridgefield, Connecticut. She has written one book already on Catherine of Aragon, which was, according to the dust

jacket, widely acclaimed. Now she has written a second, telling the story of Henry's three children from the years 1533-58. That there will soon be a third one can hardly doubt, for the last line of the present book reminds us that we have only just reached "the time of Gloriana, as she liked to be called and . . . the greatest moment in the nation's history." Unless the Darien, Connecticut, librarian or the officers and directors of the Mercantile Library in New York City (two favorite haunts of Mrs. Luke) prove suddenly ungrateful, we are bound to have a trilogy.

This is vulgarization at its simplest, the "taste for Tudors" at its most unsophisticated. Mrs. Luke thinks that a plethora of material means the documents as related by Agnes Strickland (her chief authority) or by such later masters of popular biography as Frederick Chamberlin, Richard Davey, F. A. Mumby, and J. M. Stone. She parades her erudition discretely, it is true, but bibliography and footnotes at the end tell all. Apparently, she has read some Froude and one work by Pollard, although the *Encyclopedia Americana* seems to have been as useful to her as either for background. Is it surprising, then, that she can contrast Mary Tudor and Elizabeth as the spokesmen of Renaissance versus Reformation? Needless to say, a complicated year like 1549 eludes her completely. She is better on personalities—but not a great deal. The girls come off fairly well, but the great men—Henry, Somerset, and Cranmer—are unintelligible. The "dimensional life" promised by the dust jacket for Mrs. Luke's characters rarely exceeds two.

Perhaps all might be excused if Mrs. Luke had sacrificed the academic virtues to vivid narrative. But her style is mechanical, and the story lacks focus and direction; it is also too long. In the end it is hard to see why the book was written or why it will sell. But clearly Mrs. Luke enjoyed it ("it took her to England where she visited the places the young Tudors had lived . . ."). Perhaps the taste for Tudors is invincible. Melinda, however (to judge from the preface), seems to have had her doubts, and Agnes Strickland, poor lady, would be quite astonished at the casual labors of her twentieth-century successor.

JOSEPH M. LEVINE
Syracuse University

DENNIS BRAILSFORD. *Sport and Society: Elizabeth to Anne*. (Studies in Social History.) Toronto: University of Toronto Press. 1969. Pp. vii, 279. \$7.50.

The social history of play is an important and sadly neglected subject that will be modestly enriched by Mr. Brailsford's work. He intended to make a "preliminary exploration" of "attitudes towards sports, games, exercise and physical education" in this period, and he has done so. The chief merit of the book may well lie in the questions it raises and the more detailed investigations it encourages other scholars to make. As for Mr. Brailsford's own work, it is probably most useful for the Interregnum and Restoration—periods that have received comparatively little attention from the point of view of sport.

The author is of course concerned with the degree of influence exerted by Puritanism in those years. Mr. Brailsford makes a good case for his assertion that such influence was substantial although indirect, unintentional, and largely negative. Attempts by Puritans to ban specific sports and amusements, even when backed by the force of government as they were during the Interregnum, usually failed. But where Puritans failed, Puritanism of a much broader and more diffuse type succeeded. It helped destroy much of the traditional rural sport that had been associated with the practices and attuned to the rhythms of the state church. It undermined the ancient mystique of monarchy, and in a practical way, by helping to remove the monarch, removed the occasion as well as the justification for many forms of courtly and aristocratic play. Furthermore, by taking sport seriously, if only to condemn it, the Puritan writers helped prepare the way for a more serious intellectual consideration of its aims and content by other and more sympathetic writers.

Mr. Brailsford communicates to the reader his sense of physical activity as part of a much larger social, political, and even religious whole. The subject is incredibly complex, which makes it all the more necessary for the scholar not only to survey the whole but to examine with precision each of its component parts. Unfortunately this author has not always shown such precision. He has, for example, used such terms as "sport," "game," "amusement," and "play"

almost interchangeably, although as Huizinga and others have indicated, they imply distinguishable and highly significant differences. Making clear some of the distinctions that define the differences would have been enormously helpful not only to other scholars working in the field, but to the reader attempting to disentangle the strands of Mr. Brailsford's discussion. Nevertheless, despite some serious shortcomings, this is a useful and interesting work.

PATRICIA-ANN LEE
Skidmore College

DAVID LITTLE. *Religion, Order, and Law: A Study in Pre-Revolutionary England*. (Torchbook Library Edition. The Library of Religion and Culture.) New York: Harper and Row. 1970. Pp. v, 269. Cloth \$7.00, paper \$2.95.

In *Religion, Order, and Law* David Little re-examines Weber's theories on the relationship between religion and capitalism in late sixteenth- and early seventeenth-century England, and he concludes that Weber's ideas still have considerable merit. He finds especially useful Weber's theory that traditionalistic society (where one institution dominates all others) was replaced in the early modern period by a more legal-rational society (where several institutions have autonomy) that was definitely linked with rational capitalism's replacement of patrimonial capitalism. That achievement could only occur when a distinction was made between the past and the future. According to Little, Calvin implied that kind of distinction and thus unconsciously proclaimed a new order, a legal-rational society, by his emphasis on two distinct kinds of law—civil and Biblical. Many of the ideas central to Calvin's theology—voluntary participation in the new order, equality among believers in this world, and dedicated work habits—were conducive to economic development, although that was not his conscious intention.

Little accepts the thesis of Ephraim Lipson and John U. Nef that it was a time of industrial revolution, a thesis very much under question by more recent economic historians. It probably would have been more consistent with his stress on the unconscious and sometimes contradictory forecasts for the future to be found in some theological and legal writings if Little had been

more moderate in his assessment of the actual economic development of this period.

Little selects Thomas Cartwright as representative of radical Puritan thought and William Perkins as representative of the more moderate Puritan thought, acknowledging that Perkins called himself an Anglican in his later years. Like Calvin they too wanted to establish a new order by making the state distinct from the Church. Perkins' imagery, like Calvin's (less the case for Cartwright), is very much related to economic aspects of life. They all stress the uniformity of treatment in the new order and the functionalization of tasks according to their contribution to common welfare. All these views seem to indicate a move away from a traditional ordering of society toward a more legal-rationalistic one, although these three men might have been shocked that a legal-rationalistic order would mean that the here and now became of greater concern than the afterlife.

Before going into the Anglican view, Little suggests not only for England but also for other areas where Calvinism took hold that the major characteristics of Calvinism—separation of Church and state, emphasis on work, voluntary consensual action, equality of treatment, functionalization of tasks—developed differently depending on the historical circumstances. This thesis deserves intensive study, for it may provide an explanation for differences in economic and political development between England and Scotland, for example.

In contrast to these three theologians, Little finds that John Whitgift and Thomas Hooker emphasized—in this they were typical of Anglican views—the maintenance of the traditional order that was primarily in the hands of the “earthly political-legal authority.” Much more comprehensive and systematic analysis of many more writers is needed, however, before Little's conclusions about these five men can be taken as representative of typical Anglican and Puritan views.

In the next section Little tests his thesis that concepts were changing not only among religious groups but elsewhere. In the eyes of contemporaries “tension between the more or less pure traditionalism of the lawyers and the patrimonial traditionalism of the Tudors and Stuarts” was evident. But when their views are

analyzed, what seemed to be traditionalism in the lawyers' views turns out to be the same curious “mixture of ancient precedent and original innovation” that was apparent among the Calvinists (pp. 171–72). In an analysis of the views of Anglican Sir Edward Coke on economic restraints, Little finds the idea of “voluntary labor” for all and a corresponding dislike for the royal monopolies because they singled out only certain people for labor. Unfortunately, Little seems to have overlooked an article by Barbara Malament—“The ‘Economic Liberalism’ of Sir Edward Coke,” *Yale Law Journal*, 76 (1967)—in which she takes to task all those who find Coke an economic liberal. Although Little's interpretation of certain cases needs revision, he emphasizes the contradictory quality of Coke's ideas and never depicts him as a conscious economic liberal. Even Miss Malament does not completely discard the idea that Coke could be considered as a connecting—completely unconsciously so—link between patrimonial capitalism and rational capitalism.

I join Robert N. Bellah, who wrote in his preface to *Religion, Order, and Law* that Little should be praised for his multisided and sensitive approach to this period. Beyond some significant revisions of Weber's thesis, Little's study suggests a most important level at which to conduct research—beneath the immediate conscious and more easily discerned level of activity. If pursued, this kind of research might lead to a more integrated explanation of the English Civil War, instead of the single-cause approach that has been so characteristic of many previous studies. Appended to his book David Little has given us a very comprehensive bibliographical essay including comments on most of the major works pertinent to his topic.

MARTHA ELLIS FRANÇOIS
Northeastern University

VIOLET A. ROWE. *Sir Henry Vane the Younger: A Study in Political and Administrative History*. With a foreword by DAME VERONICA WEDGWOOD. (University of London Historical Studies, Number 28.) [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York, 1970. Pp. xiii, 298. \$12.00.

A principal difficulty facing the biographer of Vane is the almost complete absence of private

letters. His parliamentary speeches have been preserved, as have his official papers as treasurer of the navy. He was the author of several important tracts. The paucity of familiar letters, however, necessitates a heavy dependence on records and on accounts by his contemporaries. Thoroughness of research in this class of materials is an outstanding feature of this book. Contemporary pamphlets and diurnals have been studied with much care. Parliamentary diaries are extensively employed, and by going to the original manuscripts Violet Rowe has been able to correct the printed versions at certain points. If gaps in the record lead her to speculate on Vane's role at various stages, the speculation is always informed and documented.

Throughout the book much attention is devoted to tactics in Parliament: committees, bill drafting, and the identity of tellers for the ayes and noes are investigated in great detail. Vane emerges as a master of these arts, with exceptional capacity for work, and ruthless, even unscrupulous, toward his opponents.

Vane's strenuous labors for the Commonwealth were coupled with great influence from 1649 to 1653, chiefly in foreign affairs and in the management of the navy. Two chapters on the Admiralty at this time are a distinct contribution to administrative history. They amply support Algernon Sydney's assertion (in a sketch of Vane reprinted here from manuscript) that Vane "was an absolute Master of the Naval Affairs" (p. 279).

Vane was in the political wilderness from the dissolution of the Rump until the death of Oliver Cromwell. In 1659 he re-emerged to take a leading position in the opposition that brought down the Protectorate. His behavior at this period was so erratic, however, as to suggest "a certain hysteria, indicative of some mental imbalance" (p. 202). Coupled with this was an arrogance that antagonized many around him. As a correspondent wrote, "Sir Henry Vayne lookes uppon the nation as unacquainted with its own good, and unfit to be trusted with power . . . he would have some few refyned spiritts (and those of his owne nomination) sitt at helme of State together with the Councill, till the people be made familiar with a Republique and in love with it" (p. 224). Vane's strengths as well as his shortcomings are thus

brought out in this exceptionally thorough study.

P. H. HARDACRE

Vanderbilt University

RICHARD BARBER. *Samuel Pepys Esquire: Secretary of the Admiralty to King Charles & King James the Second.* [Berkeley and Los Angeles:] University of California Press. 1970. Pp. 64. \$3.95.

This little book was originally designed to serve as a catalog for the Pepys Exhibition held at the National Portrait Gallery in London in 1970. The Exhibition, we are told, offered a "portrait in time," tracing Pepys's physical appearance through nearly four decades, and a "portrait in space," giving some idea of what it would have been like to be his companion about 1670. It consisted of ninety exhibits. These included around a dozen portraits of Pepys, others of his contemporaries (especially royal personages and courtiers, statesmen and stage personalities), and a variety of representations of the contemporary scene. They are reproduced here (more than twenty in color), affording an illustrated supplement to the new Latham-Matthews edition of the *Diary*. Barber has provided four essays on various aspects of the subject. The first, dealing with the likenesses of Pepys, is, within the limitations of space and the book's purpose, a comprehensive and critical account. Next comes "Great Changes Here," in which Pepys's part in the Restoration and the launching of his career are described. Here those who are familiar with Pepys and his age will find little that is new. But the last two essays, bearing on Pepys's interest in the theater and providing a reconstruction of his household, illuminate aspects of his life less intensively treated elsewhere, and (like the first essay) can be read with profit and pleasure by expert and amateur alike. As is to be expected, Barber has drawn primarily on the *Diary* for references and quotations, but the essays reveal a substantial acquaintance with Restoration England. They are gracefully written, and the physical make-up of the book is singularly attractive. It should serve to whet the appetite even further for forthcoming volumes of the new *Diary*.

WILLIAM L. SACHSE

*University of Wisconsin,
Madison*

N. T. PHILLIPSON and ROSALIND MITCHISON, editors. *Scotland in the Age of Improvement: Essays in Scottish History in the Eighteenth Century*. Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1970. Pp. vi, 270. \$8.75.

The crucial problem for writers on Scottish history after the parliamentary union of 1707 is to define their subject. Some historians have treated eighteenth-century Scotland as North Britain, an odd and provincial part of England. Others, more nationalistic, have underplayed the union and concentrated on features of national life remaining distinctively Scottish or on dramatic events in which some Scots opposed the authorities of the United Kingdom. The editors of this volume have renounced both extremes in collecting ten essays that examine major features of Scottish life in order to see how Scots adapted themselves and their traditions to the problems and opportunities created by the new situation. Scotland after 1707 is depicted as a genuine nation, but one whose political and economic life was transformed by an enlarging incorporation. It is the general conclusion that "a whole ruling class . . . can be seen trying to adapt a given social, economic, political and ideological infrastructure to promote economic growth and social progress." In the process they developed an Enlightened faith in the ability of men to exploit their environment for the general good.

In support of this theme the essays emphasize the role of the landed oligarchy, the natural leaders of Scottish life. One is not too surprised to discover that such men had ambiguous reactions to the union and that the struggle between Scottishness (pride, traditions) and assimilation of English standards and values freed a few of them to enunciate new insights that were influential far beyond the Border. It is, indeed, shown that the union stimulated Scots to interact with a much wider circle of influences and relationships than earlier independence had permitted.

The book is arranged so that there are three main units. The first of these illuminates the growth of political stability through articles discussing various relationships between the House of Argyll, the Highlands, and the government in London. The result is informative but hardly exhausts either these subjects or political

life in Scotland. Similar remarks can be made about the units dealing with the gentry and with the effects of the union on Scottish religion, education, and law. Since these are ten independently written essays, there is a certain amount of repetition, overlapping, and disagreement, but these should not obscure the fact that the authors provide valuable insights about several aspects of Scottish life and challenge some accepted interpretations. It appears that this was the purpose of the publication and that the editors hope to stimulate discussion of a period that they consider misrepresented in general works and neglected as a specialization. There are some indications that they project a more extensive and systematic treatment themselves. This preliminary volume serves as a useful introduction.

J. WILSON FERGUSON
Russell Sage College

ELIE HALÉVY. *The Birth of Methodism in England*. Translated and edited by BERNARD SEMMEL. Chicago: University of Chicago Press. 1971. Pp. ix, 81. \$6.00.

For some years now there has existed what might be called a "Methodist Club" among modern English historians. Its members neither pray together nor do they attend the sick and prisoners. Even if they wished to engage in those edifying activities, geographical distance would make it impossible to carry them out jointly. For their number is spread far and wide throughout universities in Britain and North America. What earns them the name is the fact that they share awareness of Elie Halévy's two brilliant articles in the *Revue de Paris*, entitled "La Naissance du Méthodisme en Angleterre" (Aug. 1, 15, 1906, pp. 519-39, 841-67). Whenever the subject comes up in the classroom, they can knowingly refer students to these articles, in the sure faith that a historiographical treat will await them. The trouble up to now has been that some students do not read French well enough to understand the articles; and that, more often than not, that particular volume of the *Revue de Paris* is in the hands of a member of the club who is reluctant to take it back to the library. All that has now changed, thanks to Professor Semmel's labor of love. Here are the two articles, felicitously translated, available to all

who can afford the inordinately high price. The next step must surely be to put them in paperback. As a bonus, there is Professor Semmel's excellent introduction, which looks at the essays in the context of Halévy's total achievement and surveys the more recent literature on the subject.

Halévy's starting point in 1906 was that the way in which historians had, up to that time, explained the Methodist revival of 1739 was entirely too simple; for they usually attributed it to "the influence of a few individuals endowed with a genius for leadership and organization, men sufficiently fervent, eloquent, and energetic to alter, in lasting fashion, the consciousness of an entire nation." Halévy's purpose was not to diminish the importance of what the Wesleys and Whitefield felt and did, but, rather, to show that those feelings and actions bore a relation—in part necessary, in part accidental—to certain conditions, trends, and events that in the last resort enabled these individuals to exert such immediate effects and to produce such momentous historical consequences.

Thus it was simply not the case that before the advent of Methodism the English popular consciousness was characterized by irreligion and immorality. There were, after all, the various societies formed within the Church of England at the beginning of the eighteenth century for the edification of its members. Even more important, there were the Dissenting sects, many of whose ministers had defected or become indifferent by 1730, but whose congregations had, in large part, retained their Puritan piety. A religious awakening could not spring from either alone. But the elements were there, and Methodism, "the High Church of Nonconformity," combined them effectively. Other influences must also be taken into account: the evangelical zeal of the Moravians; the pioneering preaching methods of Griffith Jones and Howell Harris in Wales; above all, the commercial and industrial crisis of 1738, which drove Walpole into war with Spain and led to his downfall and which created popular unrest precisely in those regions first set aflame by the Methodist revival. Why, then, did not that unrest lead to a revolutionary movement? Halévy's answer here is that because the English middle classes were, on the whole, religious

and conservative, Methodism "bent the popular impulses of 1739 to the form which most favored the respect for and maintenance of existing institutions." It was this answer that a few years later was expanded by its author into the famous "Halévy thesis," which argued that England was saved from revolution on the French model because of the stabilizing influence of evangelical religion.

That thesis is, of course, still a matter for controversy; and so are these essays of 1906, which should now be read in conjunction with J. D. Walsh's essay on "The Origins of the Evangelical Revival" in G. V. Bennett and J. D. Walsh, eds., *Essays in Modern Church History* (1966). But what makes them so impressive, indeed so exciting, even today is not merely the fact that they argue a case. They are models of how to write historical essays, leading the reader step by step and by means of ever more probing questions along the same path of historical investigation followed by the author, and leaving him literally in suspense until their conclusion. That is why their publication in this form will happily lead to a major expansion of the "Methodist Club." It is hard to imagine how any readers can remain unconverted.

JOHN CLIVE

Harvard University

GARLAND CANNON, editor. *The Letters of Sir William Jones*. In two volumes. New York: Oxford University Press. 1970. Pp. xlv, 452; 453-977. \$40.00 the set.

This is a memorial of scholarship to scholarship. Sir William Jones was in most respects the founder of British "Oriental," that is, Indo-Persian, studies, and one of the first of that early group of Anglo-Indian administrators (Warren Hastings was another) who set themselves to understand and preserve the native Indian higher cultures. Within the span of a relatively short life he was, *inter alia*, F.R.S., a luminary of Johnson's Club, barrister, commissioner of bankrupts, high court judge in Bengal, radical pamphleteer in the heroic generation of the American Revolution, founder of the Bengal Asiatic Society, and impressive scholar in Persian, Arabic, Sanskrit, Hebrew, Greek, Latin, French, Italian, and Spanish. When he died in 1794 at the age of forty-eight,

he was engaged in codifying Hindu law on the model of Justinian's *Institutes*.

The record is impressive. So is Mr. Garland's authoritative collection of his letters—596 letters (304 of them published for the first time) of which 22 are in Latin, three in French, and one each in Persian and Arabic. The editing is meticulous, even lavish. While the book does not quite accomplish the editor's purpose of establishing Jones as one of the great letter writers of the eighteenth century ("comparable with Walpole or Chesterfield"), it re-establishes its subject as one of the great intellectual lights of the century, and makes the details of his fascinating career available both to Asianists and students of eighteenth-century British politics.

JOHN NORRIS
University of
British Columbia

DAVID SPINNEY. *Rodney*. Annapolis: United States Naval Institute. 1969. Pp. 484. \$14.50.

If Admiral Sir George Rodney had not existed, historians of eighteenth-century Britain would want to invent him. He played the role of the ambitious aristocrat with magnificent exaggeration. His aims were to walk with the great, live lavishly, and acquire a vast estate. Nothing less would do. Did he gamble? Naturally, and with heavy losses. Debts? When his creditors closed in he fled to France. Parliament? To him a seat was more than a shield from creditors. "A man in our country," he wrote, "is nothing without being in Parliament." Patronage? He had no qualms about pleasing "the great families" with naval appointments for their offspring. Nepotism? He made his son a post captain at fifteen. As for prize money, his greed at St. Eustatius, and its naval consequences, put a large blot on his reputation. Ministers found his requests unceasing and insatiable. The Navy Board regularly found his accounts unacceptable. He was forever overreaching. This new biography gives us something of great value—a rich and engaging portrait of a fascinating admiral.

On the naval side, where nearly all the significant material was unearthed long ago, the book is less remarkable. The author's decision to appoint himself defense counsel for his sub-

ject and to shore up Rodney's naval reputation on all fronts—including that of strategic analysis, where Rodney's talents were nearly nil—makes one wonder about his critical acumen. For example, Rodney's refusal to allow Hood to take sensible measures for intercepting de Grasse in April 1781 is defended with reasoning that borders on casuistry (p. 372).

Rodney was not a well-rounded admiral, and it is both futile and historically counterproductive to exhibit him as such. He was not a good strategist. He was not a sensitive leader; sooner or later he lost the confidence and trust of all his immediate subordinates. He had no head for tactical planning, and his greatest victories were the product of courage, experience, improvisation, and luck. He was, as this book amply attests, a brave and indomitable man—as bold in battle as he was at cards. He always put his flagship in the thick of it and expected his captains to do the same. Any who did not faced court-martial and ruin. In effect Rodney was betting on English gunnery and seamanship. These were the best bets he ever made; they earned him his fame. We should take the man as he was, so that we may recognize the role that his frailties played in his successes. After all, without his debts and his dreams, would Rodney have shunned half pay and put himself so persistently on the firing line during the American war?

DANIEL A. BAUGH
Cornell University

KENNETH BOURNE. *The Foreign Policy of Victorian England, 1830-1902*. New York: Oxford University Press. 1970. Pp. xii, 531. Cloth \$12.00, paper \$5.75.

Collections of diplomatic documents for students seem to have become a staple of publishers' lists, though the intended audience is somewhat difficult to define. This compilation will prove most useful to students who are already familiar with the main outlines of British diplomacy and who have some knowledge of the main participants. The first part contains a descriptive survey of British foreign policy between 1814 and 1914 and is followed by a generous selection of documents intended to illustrate the ideas, decisions, and criticisms that shaped Victorian policy. The two sections should be used together as the early chapters

provide the necessary framework and continuity for what can only be a sampling of pragmatic responses to a constantly altering world scene.

The special value of the book lies in Dr. Bourne's assimilation of the flood of new monographic work in this field. Even the specialist will find his critical bibliography worth consulting. A wide geographic range is covered, and though somewhat uneven in his treatment of the extra-European world, Dr. Bourne excels in his description of Anglo-American relations. The author's general thesis is that freedom from commitment was the basic recurring theme in Victorian foreign policy, and the documents in part underline this contention. Unfortunately, the author's more general points are somewhat submerged in the subsequent descriptions of the policies of successive foreign secretaries assisted or checked by their cabinet colleagues. The documents, too, though selected from a wide variety of sources, only occasionally depart from the essential diplomatic or strategic outlines of the story. Despite Dr. Bourne's interesting introductory remarks little attention is paid to the effects of an increasingly competitive economic situation on late Victorian diplomacy, particularly in China. Nor, apart from Bright and Cobden, does the author illustrate the role of "the troublemakers" in defining Victorian attitudes.

This is an informative guide for the central decades of the Victorian period. The period from 1830 to 1874 is the most successfully presented and illustrated. Thereafter the patterns of diplomacy become increasingly complicated, and some gaps in coverage are inevitable. The epilogue suffers visibly from the lack of a general study of the Grey era, and the few documents included have been selected primarily to underline the author's thesis.

ZARA STEINER

New Hall, Cambridge

GEORGE CORNEWALL LEWIS. *Remarks on the Use and Abuse of Some Political Terms*. A facsimile of the 1832 text with preface and introductory essay by CHARLES FREDERICK MULLETT. Columbia: University of Missouri Press. 1970. Pp. 69, xxxii, 264. \$8.00.

Nineteenth-century England contained many shadowy but significant figures of the second rank whose historical usefulness has not been

thoroughly explored. Sir George Cornewall Lewis (1806-63) is an excellent example of the type. As a liberal Whig he enjoyed a varied and successful public life, serving as poor law commissioner, secretary of the board of control, secretary of the treasury, chancellor of the exchequer, home secretary, and secretary of state for war. He sat in Parliament for a total of thirteen years, and for three years he was editor of the *Edinburgh Review*. Lewis combined with service to the state an impressively productive career in scholarship. He was a competent classicist, an authority on the astronomy of the ancients, a philologist of considerable merit, a political philosopher, a constitutional theorist, a political historian, and a translator from German.

In his admiring sketch of Lewis, Walter Bagehot rightly emphasized the centrality of the critical element in his thought and work. In neither scholarship nor public administration was Lewis creative. Patience, precision, wide learning, and common sense were his leading characteristics. Bagehot is wrong, however, to believe that Lewis was merely an embodiment of disciplined, detached common sense. Were that true he would deserve his obscurity. His importance as a historical figure is rooted in the fact that he shared views, assumptions, and prejudices with less articulate contemporaries. Bagehot thought "there was nothing *very* remarkable in Sir George Lewis." That is why he is a useful figure for historians.

It is pleasant to have in print this well-done facsimile edition of the *Remarks* with Charles Mullett's richly informed and philosophical introductory essay. Lewis intended that this book, which he wrote in 1832, would be a kind of technical dictionary that would help introduce precision into the language of politics. In defining such words as "government," "constitution," "sovereign," "monarchy," "democracy," and "representation," Lewis produced, however unwittingly, not an objective lexicon but a political argument for the Reform Bill. Unfortunately, there is little in this edition to help the uninitiated reader place the *Remarks* in their political setting. The value of the *Remarks*, like the significance of the life of their author, can be appreciated best in their most immediate context.

R. J. HELMSTADTER

University of Toronto

EDWARD GILLETT. *A History of Grimsby*. New York: Oxford University Press for the University of Hull. 1970. Pp. x, 322. \$9.95.

In the last chapters of this general survey, Gillett, honorary archivist to Grimsby Corporation and deputy-director of the Department of Adult Education in Hull University, describes with warmth and without sentimentality Grimsby's change of character in the nineteenth century: the borough grew from a semirural village of 3,700 inhabitants in 1841 (having already enjoyed a period of growth from its mid-eighteenth-century population of about 700) to a town of 42,000 in 1881. The two decades from 1841 to 1861 were critical. Grimsby's development had been inhibited by poor inland communications. Railroad lines connecting Grimsby with Boston and with Lincoln were undertaken in 1845 and opened in 1852. The Manchester, Sheffield and Lincolnshire Railway amalgamated with the dock company and began work in 1846 on a new dock, which was completed in 1852. Grimsby's trade with Northern European ports, based on imports of timber and exports of coal from the south Yorkshire fields, increased markedly: the number of overseas ship clearances was 41 in 1844 and 722 in 1854. Later development, however, was based mainly on the North Sea fisheries rather than overseas trade. Railways were once again the key: "it was the construction of the Great Northern Line to London which really made the town a fishing port." The M.S. & L.R. opened a new fish dock in 1857, when up to fifty smacks were using the port. Fifteen years later some 110 cod liners and 371 trawling smacks were sailing out of Grimsby, and by this time, Gillett estimates, probably half of the inhabitants of the town gained their livelihood from fishing. Fishing (with steam trawlers since the 1880s) is still the town's largest industry, but food processing and freezing plants, chemical works, and heavy engineering have lessened the town's dependence on the fish trade. Most of the overseas shipping has moved to deepwater docks at Immingham, which opened in 1912.

This story, in the last seven chapters of the book, of Grimsby's nineteenth-century growth is an exciting one for which the five tediously fragmentary chapters on the medieval port town and the six extremely locally focused chapters on the silted-up, depressed, agricultural, gentry-ridden village of the sixteenth through eight-

eenth centuries might well have been curtailed. Chapters on Grimsby and the Reformation and the Civil War add little to those larger subjects. For the three centuries before the 1790s, when they gave fruitful attention to improvement of the dock, Grimsby men were mainly concerned in their public lives with a single interest: the profits to be made through freedom of the borough from the rights in the common lands and from parliamentary electioneering. Gillett's treatment of the latter subject forms the most interesting parts of the book prior to those chapters that deal with the nineteenth century.

ROBERT G. LANG

University of Oregon

STEPHEN E. KOSS. *Sir John Brunner: Radical Plutocrat, 1842-1919*. (Conference on British Studies, Biographical Series.) New York: Cambridge University Press. 1970. Pp. xii, 314. \$13.50.

The new biographical series of the Conference on British Studies is off to an auspicious start. Its first volume is a modest yet most satisfying study of one of the last of a group of Radical politicians whose stations in life varied from the trade-union bureaucracy of a George Howell to the industrial plutocracy of Sir John Brunner himself. Brunner made his mark as a chemical tycoon first in Cheshire and then in the wider world served by Brunner, Mond and Company; he gained his political reputation as a Gladstonian Liberal turned Radical who presided for some of the later years of his life over the rather hapless National Liberal Federation. Like most seemingly simple, self-made men, he was a complex personality. He sometimes managed to confuse his—or his firm's—economic interests with those of society as a whole. Occasionally he mistook the rhetoric of advanced social thinking for the tough political arrangements needed to implement well-intentioned sentiments. Yet his radicalism—so shocking to his propertied fellows in Cheshire, who could not understand the troublesome notions of a man whose great wealth should have assured a more sympathetic feel for privilege and monopoly—was perhaps more revealing of the issues of his time than were the political notions of many more prominent figures. On Irish Home Rule, the eight-hour day, educational reform, above all on naval disarmament he fought for his principles with militancy, sometimes with a dash

of the absurd, often with more than a hint of authoritarian temper, but always with courage and honesty and a genuine concern for the welfare of his constituents, his employees, and his fellow Englishmen in general.

All of this is detailed by Stephen Koss with the balance and good sense that seems to me to have informed his recent studies of John Morley and Lord Haldane. He has used not only correspondence, letter books, and family memorabilia in the possession of his subject's grandson, but a wide range of other manuscript materials from the voluminous Gladstone Papers to those of R. C. K. Ensor and J. L. Hammond. We have here a model brief life perceptively displayed by one of the ablest young historians of modern England. If the succeeding volumes published under the auspices of the Conference on British Studies match the quality of the first, the series will more than fulfill the hopes of its sponsors.

HENRY R. WINKLER
Rutgers University,
New Brunswick

R. K. PUGH, transcribed and edited by, with the assistance of J. F. A. MASON. *The Letter-Books of Samuel Wilberforce, 1843-68*. The Oxfordshire Record Society, Volume 47. [Oxford:] Buckinghamshire Record Society and the Oxfordshire Record Society. 1970. Pp. xviii, 438.

The diligence of county record societies is a constant boon to historians of England. This meticulously edited volume of the letter books of Samuel Wilberforce, published by the Oxfordshire and Buckinghamshire societies, is proof of the point. The letters Wilberforce wrote as archdeacon of Surrey and as bishop of Oxford reflect his busy life and his protean interests. Hence they mirror as well general patterns of Victorian society and the Victorian frame of mind and so will interest a wide range of historians.

There is abundant evidence here of religious stress and strain. Wilberforce—described accurately by the editor as a High Church Evangelical—battles through the storms of Tractarianism and ritualism. R. D. Hampden, the bishop's Broad Church enemy, and F. D. Maurice, the bishop's Broad Church friend, both compel him into action, while the editors of the *Evangelical Record* carp at him for moving

beyond the narrow faith in which they have embalmed his father. The materials bring religious controversy to life. Evangelicalism emerges from the confines of the missionary hall. The Oxford Movement leaves Oxford for the countryside.

That countryside remained in the hands of the Church long after the Church had lost the cities. Much credit for the fact belongs to men like Wilberforce, whose passionate care infused Victorian parishes with religious life, if not religion. The letters show Wilberforce combating drunkenness and absenteeism among his clergymen, pressing for church building and school teaching, above all working to achieve what he conceived to be his primary mission, the saving of souls.

What Woodforde's diary is for the history of the English Church in the eighteenth century, these letters are for that same institution in a far different age.

STANDISH MEACHAM
University of Texas, Austin

A. TEMPLE PATTERSON, editor. *The Jellicoe Papers: Selections from the Private and Official Correspondence of Admiral of the Fleet Earl Jellicoe*. Volume 1, 1893-1916; Volume 2, 1916-1935, with an appendix on the papers of Vice-Admiral J. E. T. Harper. (Publications of the Navy Records Society, Volumes 108 and 111.) [London:] the Society. 1966; 1968. Pp. xi, 315; xiv, 497. 50s. each.

Specialists in naval history will welcome this generous selection from the Jellicoe Papers at the British Museum, supplemented by other private and official correspondence.

The bulk of the collection is devoted to the war years. By 1914 Fisher's long-time protégé and heir-apparent to the command of the main battle fleet was fifty-five, perhaps already, as Professor Patterson hints, just past his peak for executive and command functions. The battle of Jutland proved a disappointment, and in his subsequent position as first sea lord, Jellicoe seemed—at least to his political superiors—lacking in creativity and responsiveness. Despite later abortive negotiations for a position as "Admiralissimo" of the Allied naval forces in the Mediterranean, Jellicoe's dismissal as first sea lord in December 1917 marked the end of his wartime major responsibilities. Neither he nor Fisher was invited to the intern-

ment ceremonies of the German High Seas Fleet in November 1918.

The material on 1914-18 adds much new detail to an essentially well-known story. On the postwar years, which seem to have been happier and more fruitful ones for Jellicoe, the material has, at least to this reader, the freshness of novelty. The main items are Jellicoe's naval "empire mission" of 1919-20 and his comparatively subdued involvement in the Jutland controversy. An appendix contains an indignant "statement of facts," declared before a commissioner for oaths in 1935, by then Rear-Admiral Harper, on the Admiralty's alteration and long suppression of the record he had in 1920 been ordered to make of the battle of Jutland. Lord, how these admirals loved one another!

Professor Patterson's comments are balanced, incisive, and informative.

PAUL GUINN
*State University of
New York, Buffalo*

DEREK H. ALDCROFT. *The Inter-War Economy: Britain, 1919-1939*. New York: Columbia University Press. 1970. Pp. 441. \$12.50.

This is a study of the working of the British economy over a period of twenty years. They happen to have been the years between the European wars. As they were full of economic change and double-faced events, they are worth considering for their own sake.

Dr. Alcroft, who writes from the University of Leicester, has chosen, very reasonably, not to write a general narrative. He has instead given the book the form of a study of the economic dynamics of the period, concentrating upon main causes of instability and change. He begins with a brief reference to the general trends and statistical outline of the period, then plunges into the big variations in the rate of economic change, both over the years, in the shape of trade fluctuations, and geographically, in the form of regional differences and disparities. The reader is then ready for a general examination of the conditions and limits of British economic growth at that time, beginning with the basic industries of the country, the rise of new industries, and the consequences of the long-term transformation of the economy.

Transport and the service industries come in for separate attention. Perhaps the mutual relations between them and industrial growth deserve more attention than they receive in this chapter. The book concludes with long sections on the national balance of payments, the management of the economy, and the effects of economic change on welfare and incomes.

This is clear-headed, quantitative history that summarizes and groups a vast mass of information in a critical way around some of the main problems of the period. Anyone concerned with the recent economic history of Britain, and especially students, will find it extremely useful, not least as a quick way to the sources, which are scrupulously indicated. At the same time it is only fair to point out that, as a study of economic change in Britain over these vital twenty years, the book is written within very strict limits. The student will have to turn elsewhere for answers to some of the questions it raises. The economic magnitude and consequences of the two great wars, which lie on either side of the period, are left unindicated. More significant, the handling of two of the bigger events of those years, the development of the managed economy and the welfare state, suffers, it may be suggested, from insufficient attention not only to the evolution of public policy, which is only now beginning to be known from public and private records, but also to the changes of that vague but powerful force, public opinion. It is impossible to understand the slow growth of the idea of a managed economy or the distortions that surrounded the concept of a welfare state without being prepared to devote time to the vagaries and illusions even of well-informed opinion. Illusion is not only an inevitable but a necessary part of history. In the words of G. L. S. Shackle, "Man makes his history in trying to understand it." On this side, one feels, Dr. Alcroft's treatment is not general enough.

On another side, it is hardly sufficiently microscopic. For it pays relatively little attention to the actual process of economic decision, whether that is being taken by management or labor, considered as a factor in production. Business and trade union history, badly as they are often written, really have something to contribute here to our understanding of economic change. If Dr. Alcroft leads some students to

look in that direction, as well as toward economic policy, he will have done them a good turn.

W. H. B. COURT
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W. N. MEDLICOTT *et al.*, editors. *Documents on British Foreign Policy, 1919-1939*. First Series, Volume 17, *Greece and Turkey, January 1, 1921-September 2, 1922*. London: H.M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. cxxxii, 948. \$27.00 postpaid.

The documents and correspondence in this volume cover the period from January 1, 1921, to September 2, 1922, the date of the request of the Greek government to arrange an armistice with the Turkish Kemalists following the defeat of Greek forces in Anatolia. This request underscores the British failure to develop an effective Middle Eastern policy designed to assure regional stability in the wake of the Ottoman defeat. Disagreement on Anglo-French aims and divisions within the Foreign Office regarding the "Greek policy" formulated by Lloyd George further hampered effective British action, as the documentation amply shows.

In addition to a useful selection of Foreign Office documents and intergovernmental correspondence the volume includes extensive memoranda covering the private views of members of the Foreign Office and British representatives in Athens and Constantinople. Thus Harold Nicolson writes: "Greece constitutes a very positive asset in British imperial policy"—a view that carried much weight with the permanent under-secretary of state, Sir Eyre Crowe, and was supported by British representatives in Athens. At the same time Sir Horace Rumbold in Constantinople was urging a new policy toward Turkey with the sultan as the cornerstone but with concessions to the Kemalists to bring them back into the Ottoman fold. Had Rumbold's view prevailed the postwar history of the Middle East might have been shaped by surer hands under less trying circumstances.

Valuable though they are, the documents in this volume are only part of the story and must be studied in conjunction with its predecessor and especially with the forthcoming volume, which will presumably deal with the Conference of Lausanne, the withdrawal of Allied forces from Constantinople, and the establishment of

the Kemalist regime as the visible symbol of resurgent Turkish nationalism.

WILLIAM SPENCER
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ANTHONY MASON. *The General Strike in the North East*. (University of Hull, Occasional Papers in Economic and Social History, Number 3.) [Hull:] University of Hull Publications. 1970. Pp. vi, 116. £2.

Local studies, currently proliferating among historians of British working-class movements, run the obvious risk of falling into antiquarianism. This approach is, however, well suited to an examination of some aspects of the General Strike of 1926. The failure of the leaders of the Trades Union Congress to formulate strike plans in advance and the communications difficulties resulting from the self-defeating decision to call out the printers threw workers in each locality on their own resources. Anthony Mason's detailed investigation of the course of the strike in Northumberland, Durham, and the West Riding of Yorkshire is thus a valid historical enterprise.

The author has pieced together the story of the strike in the northeast from scattered sources ranging from the minutes of local strike committees to cabinet and Home Office papers. His findings tend to support the conclusions of broader studies of the strike by Crook and Symons: the workers displayed amazing solidarity, the emergency machinery of the government achieved only a limited effectiveness, and, in the days following the strike, an indeterminate number of employers instituted reprisals against returning workers.

Although Mason's account does not contradict the accepted picture of the strike as generally orderly, it contains no such jolly episode as the cricket match between police and strikers at Plymouth. Workers in the northeast, according to Mason, were disciplined rather than relaxed during the early days of the strike, and he finds mounting, if scattered, violence on the final weekend. Statistics of arrests suggest that this may represent a deviation from the national pattern, but, in the absence of comparable studies of other regions, the conclusion is necessarily tentative.

While the chief value of this monograph is its detailed corroboration of earlier studies, it provides some useful qualifications and a sur-

prisingly fascinating vignette of the response of ordinary men to Britain's greatest domestic crisis of the interwar period.

CATHERINE ANN CLINE

Catholic University of America

W. N. MEDLICOTT *et al.*, editors. *Documents on British Foreign Policy, 1919-1939*. Second Series, Volume 11, *Far Eastern Affairs, October 13, 1932-June 3, 1933*. London: H. M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. lxxvii, 597.

Appearing speedily after its predecessor, this volume covers British Far Eastern policy from the appearance of the Lytton Report to the signing of the Tangku truce, taking in on the way the nonrecognition of Manchukuo and Japan's resignation from the League of Nations. It brings out admirably, with the help of a substantial amount of internal memoranda and minutes, the various shades of opinion within the Foreign Office and among diplomats in the field: Lindley in Tokyo, vehemently against any move that might antagonize Japan; Lampson in Peking, convinced of the future greatness of China and that British interests lay "in keeping in with China and the Chinese people"; Cadogan, anxious that the principles of the Covenant should be upheld; Pratt, lengthily attempting to clarify "a middle course on which all may eventually be persuaded to unite." For Simon the dilemma between various desiderata (not simply between *Realpolitik* and idealism, though this particular tension was involved) foreshadowed the one he was to face in the early stages of the Abyssinian crisis. Immediate self-preservation predominated, of course: "We must strive to be fair to both sides. But we must not involve ourselves in trouble with Japan."

One consequence was that, as Hugh Wilson reported to the State Department, "the struggle at Geneva is seen as no longer between China and Japan or between the League and Japan, but rather between Great Britain and the League." It is worth remembering, however, that the likelihood of continuing passivity on the part of the United States was apparent in London (Norman Davis is recorded here, incidentally, opposing the notion of expelling Japan from the League); and when reading reports in this volume of, for example, Swedish denunciations of the continuing search for con-

ciliation, Scandinavian attempts in earlier years to protect their countries from the possible consequences of the sanctions provisions of the Covenant should also be borne in mind.

In any case, the comforting belief in the Foreign Office—and it was shared in varying degrees by Stimson, Drummond, Lytton, and others—was that the occupation of Manchuria would itself bring increasing hardship for Japan. Meanwhile nonrecognition of Manchukuo should not be regarded as inevitably eternal, nor should the proposals emanating from Geneva for increased technical and financial aid to China be accepted without an eye to their effect on Tokyo. The essentially defensive nature (some would no doubt say pusillanimity) of British foreign policy is well conveyed in this excellently edited volume. What the book does not and cannot be expected to show is the major and at times bitter conflict that in 1933 was gathering way in Whitehall between those who sought safety in the Far East through moving close to Japan and those who would qualify such a policy by paying attention to American susceptibilities. One of the jokers in the pack does appear in these pages, however: the growing fear of Japanese trade rivalry. Some wondrous contortions lay ahead.

CHRISTOPHER THORNE

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SIR LLEWELLYN WOODWARD. *British Foreign Policy in the Second World War*. Volume 1. (History of the Second World War.) London: H. M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. lx, 640. \$13.14 postpaid.

This is the first of a five-volume history of British foreign policy, written between 1942 and 1950 "for official use" (p. v), that is, as a reference book to be used by members of the Foreign Office. The late Sir Llewellyn Woodward had access to all British archives that he wished to use and was subject to neither directives nor censorship.

Ten years ago an abridgment of this history was published (*AHR*, 68 [1962-63]: 117-18). The thirty-eight-page introduction to that book is reprinted in volume 1 with very minor footnote changes.

The abridgment was somewhat critically received because the book was not conceived on the scale of penetration that might be ex-

pected from a historian of Woodward's achievements. The one-volume affair was mainly an account of Foreign Office information, action, and views. This limitation was restrictive to the point of misrepresentation when foreign policy was being made by many people and influences outside that department. A. J. P. Taylor, indulging his low esteem for the contents of diplomatic archives, asked why Woodward was assigned such a penitential and penal labor. Taylor's indignation was not stayed by a partial answer readily available to him: Woodward had undertaken a wartime task and had found a challenge in writing a source-based contemporary history of his country's adjustments to war and the revolutionary pace of the postwar world.

The narrative, chronologically pursued, tells us what the Foreign Office knew and thought on each occasion. This is history almost of the sort that Samuel Rawson Gardner sought to write. At any rate the straightforward stylistic presentation is unflaggingly maintained, and there is no visible evidence that Woodward suffered from his penal labor.

If Woodward did not fail, some readers will. There are *longueurs* here, even for those inured to the lavish scale of the British histories of the Second World War. The criticisms leveled at the abridgment still apply: this is a properly constitutional history, silent about much in the Foreign Office and about many other policy divisions. It is nonetheless useful and precious when used with other sources. That it is not a complete history of British foreign policy is to say no more and no less than that for the war years it could not be written, as the official histories require, on a departmental basis. At times the details in the long narrative, for example, on Scandinavia in 1939-40, make it almost a different story. But the abridgment required concision. The judgment on the project to send Allied soldiers to Finland, "The military assumptions behind this policy were remarkable," loses some of its punch in volume 1.

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JOHN J. SILKE. *Kinsale: The Spanish Intervention in Ireland at the End of the Elizabethan Wars*. New York: Fordham University Press. 1970. Pp. xvi, 208. \$10.00.

In October 1601 a Spanish force under Águila occupied the walled town of Kinsale in Munster. Instead of taking to the field Águila fortified himself and waited for reinforcements, for the Munsterites to rise, and for the rebel armies of O'Donnell and O'Neill, then in Ulster, to join him and defeat the English. He waited in vain, however, for the English under the command of Lord Mountjoy forced the Spaniards to surrender Kinsale and return to Lisbon. Relying heavily upon manuscript sources, Professor Silke renders a detailed narrative of this so-called battle of Kinsale. He devotes single chapters to military preparations (ch. 7), the disembarkation in Munster (ch. 8), the siege of Kinsale (ch. 9), and the humiliating rout (ch. 10). This portion of the work is straight military history. Since Professor Silke writes from an avowed Spanish vantage point, his fresh perspective is commendable and most welcome.

Actually, the military superstructure of *Kinsale* is smaller than and subordinate to the diplomatic substructure. The author views the Spanish defeat at Kinsale as a critical turning point in the triangular relationships between Spain, England, and Ireland. Before Kinsale, the Spanish were committed to a policy of intervention; after Kinsale, Philip III pursued a policy of disengagement that led to the Anglo-Spanish Treaty of 1604 and the Hispano-Dutch truce of 1609.

The first half of *Kinsale*, in contrast to the second, is very broad in scope. Silke discusses the evolution of the Hispanic Empire (ch. 1), the origins of intervention in Ireland (ch. 2), the imperial administration of Philip III (ch. 3), the Irish resistance under O'Neill (ch. 4), the intricacies of Hispano-Irish diplomacy (ch. 5), and the decision to invade Ireland. While these chapters serve to place the battle of Kinsale in proper perspective, some of the information is irrelevant and little is new.

Nevertheless, through extensive use of Spanish archival sources, Professor Silke has produced an exceedingly accurate account of the Spanish debacle at Kinsale. The extensive annotation and the bibliography testify to the thoroughness of the research. The maps, the appendixes, and the definitive index enhance the value of the book. Very likely *Kinsale* will, as Professor D. B. Quinn predicts in the fore-

word, "take its place as a standard treatment of the subject."

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KEVIN B. NOWLAN and T. DESMOND WILLIAMS, editors. *Ireland in the War Years and After, 1939-51*. [Notre Dame, Ind.:] University of Notre Dame Press. 1970. Pp. ix, 216. \$8.50.

World War II, postwar reconstruction, and the beginning of the cold war all had a dramatic impact on European institutions and life styles. The impact on neutral Ireland was muted but not insignificant. During World War II de Valera, the dominant political personality in Irish life, mobilized defenses against possible invasion and with scrupulous integrity and delicate yet firm diplomacy upheld his country's neutrality against British and American pressures. Neutrality determined "Ireland's right as a nation to pursue her own national interests and the power to pursue them" (p. 204). War-time shortages and inflation tested a weak Irish economy. Inadequate employment possibilities forced a stream of emigrants into the British labor market, a trend that continued after the war.

Continuing economic pressures defeated de Valera's Fianna Fáil party in 1948. An inter-party government led by Fine Gael's John Costello took office. This coalition, with its conflicting goals and personality differences, had limited success in meeting the economic challenge, but it strengthened the Irish democracy by revitalizing Irish party politics, enacted social legislation, attempted to strengthen Irish agriculture, and started to expand industrial development beyond the limits of domestic consumption. In order to ease political tensions and steal a march on Fianna Fáil, the coalition took Ireland out of the Commonwealth, an act that strengthened the walls of partition, as did Costello's decision to surrender to the Catholic hierarchy's objections to a comprehensive medical care program.

Despite the attractions of the American dollar Ireland responded to the cold war by reasserting her neutrality, arguing Britain's occupation of the Six Counties. Neutrality, however, did not mean isolation, and politicians in all parties supported efforts to achieve European political and economic cooperation.

With the exception of drama, Irish literature

during the 1940s sustained an amazingly high level of quality. Writers led the liberal intellectual attack on clericalism and conformity, with Sean O'Faolain dominating the literary scene. His periodical, *The Bell*, provided publishing opportunities for old and new talent and served as the liberal conscience of the nation.

When the coalition fell in 1951 Ireland still confronted a variety of complex problems: a stagnant and inadequate economy, migration from rural to urban centers, emigration, and delicate Church-state relations. But over a thirty year period the young nation had survived civil war, depression, political extremism, and the crisis of neutrality, and she did so with her democratic faith and institutions in relatively sound condition.

Evaluating books of essays is frequently a difficult task. Often there is too much repetition of material, uneven quality of contributions, emphasis on subjects of minor importance, and obscuring of main themes. These fifteen essays by fourteen authors, originally presented as the Thomas Davis Lectures on Radio Éireann, are exceptions to the rule. All of them are thoughtful and well-written presentations of important subjects. Some of them are of exceptionally high quality, particularly Augustine Martin's discussion of Irish literature. Nowlan and Williams have presented an important analysis of the development of Irish society at a critical time in the postrevolutionary period.

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FRANÇOIS HINCKER. *Les Français devant l'impôt sous l'Ancien Régime*. (Questions d'histoire, Number 22.) [Paris:] Flammarion. 1971. Pp. 186.

In 162 packed pages, François Hincker has compressed what is known and what is currently being studied by scholars concerning the French fiscal system in the seventeenth and eighteenth centuries. The book is the latest volume in the Questions d'histoire series published by Flammarion. It has a good, workable format: in an extended, original essay incorporating research ranging from that of Marcel Marion, still indispensable after more than fifty years, through the recent studies of popular rebellions of Boris Porchnev and Roland Mousnier and his students, to the critical investigations of Yves Durant into the phenomenon of the General

Farms, Hincker examines the operation of the seventeenth- and eighteenth-century fiscal engine within the structure of Old Regime society. Next is a collection of judiciously selected documents designed for classroom use. There follows an exploration of unresolved problems and current surrounding controversies—a fresh look at the problem of the reliability of Necker's *Compte Rendu*, an examination of the new methods being used to estimate the profits of the General Farms, a presentation of various viewpoints on the significance of popular rebellions in the seventeenth and eighteenth centuries, a re-examination of the fact and meaning of privilege in the Old Regime, a survey of learned opinion regarding the incidence and impact of royal fiscal policy upon the French economy and class structure, and a study of recent scholarship concerning the ecclesiastical *dîme*. The work concludes with a chronology, a glossary, and a bibliography containing works published as long ago as 1703 and as recently as 1970.

Hincker's book is a helpful guide through the thickets of fiscal history. He angles into the old questions from the viewpoint of popular response, not only to specific fiscal burdens, but to the whole idea of public taxation itself. He sees as endemic in the Old Regime a profound resistance to public taxation of any sort. This bias connected nearly all seventeenth-century popular rebellions; it fired eighteenth-century interest in political economy; it exploded in revolution. While this is not a new idea, Hincker shows that recent scholarship has once again recognized its importance.

GEORGE T. MATTHEWS
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GUY CHAUSSINAND-NOGARET. *Les financiers de Languedoc au XVIII^e siècle*. (École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Affaires et gens d'affaires, Number 35.) Paris: SEVPEN. 1970. Pp. 374. 50.80 fr.

A *financier*, Diderot's Encyclopedia tells us, "is a man who manages the *finances*, that is, the royal funds, who has an interest in the tax farms and in the financial affairs of His Majesty." This definition evokes the image of the famous farmer-general—enormously rich, manipulator of the royal *caisses*, speculator in tax

revenues, parasite on the royal administration—in brief, half banker, half buccaneer. The *financier* of Guy Chaussinand-Nogaret's book is something more. The term embraces a much larger group—*officiers de finance* ranging from such famous court bankers as Antoine Crozat to lesser known officials including hundreds of tax and tithe receivers, military procurement officers, and all the so-called *intéressés aux affaires du roi*. Chaussinand-Nogaret insists on the entrepreneurial function of these men. Far from restricting their business operations to the use of public funds for short-term loans, the *financiers* were primarily investors and managers of large and diverse commercial and industrial enterprises, ranging from Atlantic slave ships to textile mills and iron foundries throughout the kingdom. For these men, office and land were business assets, the marks of a sound credit standing rather than the principal sources of an income. Public and private function were inseparable.

With considerable verve Chaussinand-Nogaret presents a series of family and clan histories in which public career, business activity, marriage alliance, and even religious affiliation fuse into tightly knit consortiums of national and international investment banking. Among the most important groups were those centering around Pennautier, Bonnier de la Mosson, Peyrenc de Moras, Castanier, and Crozat. Although these *financiers* deflected part of the national revenue from productive investment by short-term speculation, the author insists that "a considerable part of it was put back into productive circulation." Without hiding examples of questionable financial integrity, the author emphasizes the cohesion, ingenuity, skill, and, above all, the range and dynamism of these *affairistes*.

Why did so many of these families originate and prosper in Languedoc before moving their operations and residences to Paris? Although apparently an agricultural backwater, Languedoc with its textile industry offered not only a favorable economic setting but also an extensive financial apparatus attached to the provincial estates and clergy. Following the lead of Herbert Lüthy (*La banque protestante en France de la Révocation de l'Edit de Nantes à la Révolution* [1959, 1960]), the author also asserts that Calvinism gave the clans more co-

hesion and offered them the international contacts and "facilities" for grasping economic opportunity. Making the best of both worlds, they made their peace with the powerful local Catholic clergy who employed their financial expertise.

By 1770-75, the entrepreneurial *financier* had passed his prime. The author sees a gradual atrophy of the entrepreneurial function, a movement toward "officialization" in the last generation before 1789. By Necker's administration, most *financiers* had become specialized "technocrats," less and less interested in the dynamic world of "affairs" and more and more concerned with privileges, status, and a cultivated life in Paris. The "brilliant *finance* of the Ancien Regime" had come to an end at least a decade before Lavoisier, that famous farmer-general, went to the guillotine.

This book offers an interesting complement as well as contrast to the recent work of J. F. Bosher, *French Finances, 1770-1795: From Business to Bureaucracy* (Cambridge, 1970). Bosher investigates the same *financiers* from the perspective of administrative efficiency and rationalized public finance. To Bosher, the *affairisme* of the *financiers* appears much less "salutary" for the kingdom.

ROBERT FORSTER

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R. C. COBB. *The Police and the People: French Popular Protest, 1789-1820*. New York: Oxford University Press, 1970. Pp. xxi, 393. \$13.00.

This is an important and fascinating book, though misleadingly entitled. It has little to do with the police except for a penetrating critique of police reports as evidence of popular protest. Its main value lies in the author's enormous knowledge of the kinds of people who became militant activists or sans-culottes in 1793. Its value will be most apparent to readers already well versed in the French Revolution, who can relish Mr. Cobb's allusive and witty manner, go along with his bilingual alternation between English and French, and accept the idiosyncracies that he almost boastfully avows. These include his detestation of sociology and "social structures," his disbelief in general or comparative history, his insistence on the concrete and the local, and his warnings that no one

should attempt to study the sans-culottes at all unless (like Cobb) he has spent many years in France and achieved a true inwardness with his subject.

The book's wider significance is in its relation to the whole trend in revolutionary historiography set by the work of Albert Soboul published in 1958. Cobb agrees with Soboul that there was in fact a great upsurge of popular revolutionism in 1793, distinct from and opposed to the middle-class Revolutionary Government. Like Soboul, he is warmly sympathetic to these militants. Like Soboul also, though more positively, he thinks that the sans-culottes, in their extreme localism and short-sightedness, represented a wave of genuine anarchy, which the Revolutionary Government had to suppress.

Beyond all this, Cobb's views are refreshingly independent. Certainly no Marxist framework, however loose, can contain them. He doubts that the sans-culottes are to be equated with any particular social position, seeing them as a "political accident" (p. 120) or a "temperament" (p. 181) rather than as economic or social beings. He is skeptical of Soboul's "popular movement," and remarks that Soboul was actually observing a microscopic elite. He thinks that popular apathy was as significant as popular militancy, and some of his best pages explore the process by which militancy receded after 1795. He considers 1795 to be the decisive year of the Revolution, since it was the Thermidorian settlement that survived. Later revolutionary memories were "mish-mash" (p. 210). Nor does he admire the Thermidorians; his guess is that over eighty thousand persons were arrested in the repression of the Year IV, and that thousands were killed in the White Terror, often by persons of the same temperament and social origin as their ex-terrorist victims. His sans-culottes of 1793 struggle less against the bourgeoisie than against the peasants, and by roving through the countryside in search of food and menacing the farmers, they brought on the opposite of what they wanted, an abrupt decline in agricultural production. They ruined their own movement by their brutal threats, and they were too haunted by the fear of food shortage to make good revolutionaries, since they had few other sustained ideas, and any government could buy them off by manipulating the food

supply. The whole last third of the book is a study of the chronic problem of "dearth," that is the fear of shortage even more than shortage itself, in which the problems of government as well as those of the poor are presented, from the *ancien régime* down to the spread of the railways.

The book is the more welcome since it is the author's first detailed work in English, his previous and even bigger studies of the same subject having been written in French. It succeeds in its main purpose, which is to make us see the *sans-culottes* as long-suffering human beings—and perhaps also to portray violence without glamorizing it as a constructive instrument of change.

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MICHAEL R. MARRUS. *The Politics of Assimilation: A Study of the French Jewish Community at the Time of the Dreyfus Affair*. New York: Oxford University Press. 1971. Pp. viii, 300. \$12.00.

Edgar Morin in *Commune en France: La Métamorphose de Plodémet* (1967) notes the reserve shown a converted Jewish doctor in this village in Finistère. A communist sailor referred to him as a Jew rather than a doctor and a bourgeois woman refused to admit she knew his birthplace—this in a community that had known no Jews and that was tolerant of a black African married to a schoolteacher.

The small Jewish community in nineteenth-century France shared the dedication of this isolated physician to the humanitarian goals enshrined in the nation's republican tradition. Grateful to a state that had granted emancipation, its major preoccupation was to reflect a patriotism worthy of the ideals frequently described as the "second law of Moses." A symbol of this official assimilationist doctrine was the donation in 1892 by Alphonse de Rothschild of the statue of Jeanne d'Arc in the Musée de Cluny. Neither the anti-Semitic outbursts of the 1880s nor the Dreyfus affair destroyed this trust that in more critical circumstances would inspire the heroism of Marc Bloch or Georges Mandel. Only isolated individuals like Bernard Lazare and a fringe of recent immigrants rejected assimilation in favor of Jewish nationalism or Zionism, both by-products of the affair.

This persistent loyalty of the Jewish community is the theme of this volume. It is surprising that the author could break new ground in the Dreyfus case; yet there has been no general study of the Jews in nineteenth-century France, and analytic histories and biographies are rare. Documentary sources are scanty, with some archives destroyed in World War II and others, not consulted by the author, located in Jerusalem. Relying chiefly on Jewish journals and contemporary reports, Professor Marrus reconstructs a narrative of Jewish reaction to the crisis that adds substantially to our knowledge of the Dreyfus affair.

JOSEPH N. MOODY
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EDWARD E. MALEFAKIS. *Agrarian Reform and Peasant Revolution in Spain: Origins of the Civil War*. New Haven: Yale University Press. 1970. Pp. xix, 469. \$15.00.

This is without doubt the most scientific study in any language of Spanish agrarian problems. The first third of the book is devoted to a detailed exposition of the complex forms of land tenure in Spain, followed by a carefully nuanced discussion of the influences of geography, history, social structure, techniques of cultivation, and nonagricultural as well as agricultural facets of the political economy of rural Spain. The remainder of the book deals with the agrarian reform efforts of the Second Republic, 1931–36, and with peasant expectations and reactions, including the roles of anarchism, anarcho-syndicalism, and Marxism, and the efforts of various political groups to exploit or satisfy peasant hopes. From a scholarly point of view the most original portions are the analyses of land tenure and social structure and the analyses of several proposed reforms of the republican era, the actual Agrarian Reform Law of 1932, and the extent of land transfer occurring both by legal methods and by revolutionary seizures. There were no fewer than three separate legislative proposals made between July and October 1931, while Alcalá-Zamora was prime minister. Three more bills were drafted during the first months of Azauna's premiership, and then during four months, May 10–September 9, 1932, the eventual Agrarian Reform Law occupied almost three tenths of the total Cortes debates. Malefakis has expounded

all these proposals and placed their provisions in historical and social perspective. He notes how contradictory were the motives of the middle-class legislators, and in the chapter concerning "The Destruction of the 'bourgeois' Republic" he points out that on one day, March 25, 1936, the organized peasants of the province of Badajoz occupied more land than had been distributed by the several Spanish cabinets since September 1932. In the course of his exposition the author has made an exhaustive critical analysis of massive statistical data that are very uneven in accuracy and completeness and that were collected for purposes different from those of a democratically oriented foreign scholar. Thus, in the appendix concerning sources of information he sighs at the failure of the cadastral reports to furnish detailed classifications of landlord income, so that it is impossible to distinguish moderately successful from enormously wealthy owners.

While the narrative is sometimes either repetitive or hard to follow, the interpretations are very clear. The *latifundio* problem is seen as dating from the thirteenth-century reconquest, at which time war captains and military orders received their huge estates from the Crown. The *desamortización* of 1837 practically expropriated the Church as a landowner and greatly reduced the proportion of noble landowners, but it simultaneously aggravated the general problem because land sales of the ensuing decade, mostly to wealthy bourgeois, concentrated land ownership in southern Spain even more highly than during the previous five hundred years. Republican efforts of the 1930s met the following obstacles. No arable state or municipal land was available. There were no Church lands, as in the French Revolution, and no significant foreign-owned estates, as in the Romania of 1918 or the Algeria of 1963. Any Spanish land reform was thus bound to injure the native bourgeoisie. In addition the Republican government wrote its 1932 law in such a way that many small middle-class owners felt threatened and therefore opposed a land reform that could and should have been directed specifically at the immense holdings of mostly absentee landlords. In discussing peasant revolutionary impulses in southern Spain Malefakis points out that the thirst to *own* land was the great motivating factor. Northern peasants

might have just as low a standard of living, but the possession of a bit of land or of a secure lease kept them from following a revolutionary leadership. The book deserves the widest readership among historians and social scientists concerned with the problems of peasants everywhere. It is undoubtedly a definitive study with regard to Spain.

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ANDERS BJØRNVAD. *De fandt en vej: Den allierede overflyvning af Danmark under besættelsen og hjælpen til nedstyrkede britiske og amerikanske flyvere* [They Found a Way: Allied Flights over Occupied Denmark and Assistance to Downed British and American Airmen]. (Odense University Studies in History and Social Sciences, Volume 1.) [Odense:] Odense University Press. 1970. Pp. 204.

Anders Bjørnvad has devoted many years to the study of Allied overflights of Denmark during the Second World War, and his book is the product of historical research plus a bit of journalistic sleuthing. The flights themselves were bombing missions to and from Poland or North German targets, courier flights, or mine-laying operations. Targets in Denmark itself were rarely attacked. The author has attempted to establish how many aircraft were involved (forty thousand), how many Allied flyers spent time over Denmark (two hundred fifty thousand), the number of flyers buried in Danish soil (twelve hundred), the number who survived a landing (three hundred), and most particularly how many found their way into friendly hands and were transported illegally to "neutral" Sweden (ninety-three). No one knows how many hundreds found a watery grave in Danish seas.

De fandt en vej ("They Found a Way") is a title that pays tribute to those Danes who dared give shelter, food, and transportation to Allied flyers in the face of warnings by their government and threats of stern punishment by Nazi military authorities. Unless one remembers that these flyers were of necessity "enemies of the Greater German Reich" one cannot sense what it meant for a Dane to have one of these downed flyers knock at his door in the dark of the night. There were, to be sure, resistance figures who finally decided to live outside of Danish law and in defiance of German author-

ity. But isolated citizens had to make a decision on the spot. Understandably and tragically, many at first opted for the law of the land rather than the tug of the heart. Only after their own government had reached an impasse with the German occupiers in the fall of 1943 and people generally had become aware of how to contact resistance fighters did a greater sense of community responsibility develop.

Some of the real heroes of the piece were men of the cloth. When dead airmen in German hands were to be buried, Danish clergymen insisted that the ceremony be properly reported and honorably performed. When they were contacted about airmen in hiding, they did everything in their power to bring them to safety.

The book itself is the product of research in official and private Danish archives, published German, British, and American records, newspaper files, interviews, and correspondence with, or accounts of, many of the participants. It is sometimes repetitive, on occasions a little dull, but it is probably as definitive a history of this subject as we need or want. And there must be something nice about an author who, in his preface, thanks his wife, Kirsten, for finding time to type the final version despite her own school responsibilities and the demands of two children and a French poodle.

ALBIN T. ANDERSON
University of Nebraska

OSMO JUSSILA. *Suomen perustuslait: Venäläisten ja suomalaisten tulkintojen mukaan, 1808-1863* [Finnish Fundamental Laws as Interpreted by Russia and Finland, 1808-1863]. (Historiallisia Tutkimuksia, Number 77.) Helsinki: Suomen Historiallinen Seura. 1969. Pp. 286.

The first decades after Finland became a part of the Russian Empire were remarkably uneventful. While the rest of the Continent went through revolutionary upheavals, Finnish spokesmen and representatives of the imperial government discussed the abstract but portentous question of the precise legal definition of Finland's relation to the rest of the Empire and to the tsar. At the center of the discussion was the problem of defining Finland's "fundamental laws" and indeed what this and related terms meant in the first place. As Mr. Jussila emphasizes in his monograph, the issues were

purely semantic. In the first years after the annexation the discussion on both sides concentrated on finding a general definition of the fundamental laws, which Alexander I had formally promised to preserve. As time passed, however, the debate centered on the Swedish form of government of 1772 and the Security Act of 1789, which, according to Finnish spokesmen, were the central documents of Finland's constitution. In the 1840s Finnish legal scholars attempted to write a formal constitution based primarily on these statutes. Their campaign ended with a partial victory: Alexander II formally recognized the relevant sections of the two Swedish enactments as Finland's fundamental laws. The story is not a dramatic one, but its implications are important, for the clashing constitutional opinions that began to emerge in the 1840s loomed large in the bitter disputes that were to erupt at the end of the century about Finland's relations with the Empire.

Mr. Jussila has approached his subject diligently, taking care to outline the various interpretations of the terms "constitution" and "fundamental laws" current at the time in Sweden, Russia, and the rest of Europe as well as in Finland. In order to avoid partisanship he has, in each part of the period under consideration, set the Finnish and Russian interpretations of Finland's constitutional position side by side and has shown that for most of the period the views of the parties were not far apart and differed, insofar as they did, largely because the two sides instinctively adopted somewhat different definitions of the basic terminology involved. By so doing the author effectively disposes of many of the cherished nationalist myths perpetrated by later generations of embattled partisans who sought in their grandfathers' debates the origins of their own views. In short his work is sound, perceptive, and most useful.

At the end of the book the author has included an extensive English summary of his arguments and conclusions.

ROBERT O. CRUMMEY
Yale University

HEINRICH BRÜNING. *Memoiren, 1918-1934*. Stuttgart: Deutsche Verlags-Anstalt. 1970. Pp. 721. DM 30.

Brüning's memoirs are one of the most important and most depressing books on German political history to have been published since the war. They are important because the last responsible statesman of the pre-Hitler era speaks to us here from the grave. He describes to us, with candor but not always without rancor, the frightening load he carried as chancellor. His memoirs will make a total re-evaluation of his stewardship imperative.

The two most significant sections of this tremendously detailed and not very polished work were dictated only a few years after Brüning's forced resignation, in 1934 and 1935, when he was an exile in Switzerland. His recollections of events and personalities were then still precise. The account was partly based on the detailed daily calendar, kept by Brüning's trusted helpmate, Staatssekretär Hermann Pünder, and on certain secret government documents. He also discussed vital parts of the book with such close associates as his vice-chancellor, Hermann Dietrich, and the former Reichsministers Gottfried Treviranus and Rudolf Hilferding, as well as some foreign statesmen like Ramsay MacDonald and Henry Stimson, whom he greatly admired. I have not found any factual error in this part, which covers events I observed as an editor in Berlin from close by. In the 1950s Brüning added to these sections an introductory part describing his military career at the end of the First World War and his rise to political influence during the first ten years of the Weimar Republic; a final section covers his role from his dismissal to his flight from Germany in 1934.

The first part especially enables the reader to gain new insights into the roots of his personality. The experience at the front as a young officer had the greatest formative influence on him. He despised the November Revolution. Although his party, the Catholic Center, occupied a key position in the whole Weimar era, he never cared much for that republic; nor was he really a dedicated Center politician. Of the Weimar leadership he admired only President Friedrich Ebert and the two Reichsministers Heinrich Brauns and Otto Gessler. He pays reluctant tribute to Gustav Stresemann, the "merits of this great man and the great perspectives of his policy" (p. 111), but clearly no love was ever lost between them. Nor did he

care much for the two leaders of his own party, Chancellor Wilhelm Marx and Monsignor Ludwig Kaas, who in later years became one of the main objects of his growing hostility. Nor does Cardinal Eugenio Pacelli, later Pope Pius XII, escape severe criticism. Brüning's state visit to the Vatican in 1931 led to an open clash between them, when Pacelli demanded the immediate conclusion of a concordat with the Reich and suggested cooperation between Center and National Socialists. Brüning contrasts this painful visit with the much more pleasant reception by Mussolini, with whom he established total rapport in intimate conversations.

Several years ago I had a four-hour visit with Brüning at his home in Vermont. He discussed with me the key problems of his chancellorship very frankly. When I urged him to have his memoirs issued during his lifetime, he maintained that they could be published only after his death, for he had to speak the truth, but it could not yet be told. He had kept this reticence consistently since 1932, but I believe his silence was a great disservice to his nation. Brüning should have spoken out clearly when it might still have had some impact. He owed the people who had worked and voted for Hindenburg's re-election the truth about the rapidly progressing senility of the president, his increasing disloyalty, his unconstitutional long-range aims, and the camarilla of which Hindenburg had become a puppet, centering around Staatssekretär Otto Meißner, General Kurt von Schleicher, Chancellor Franz von Papen, and, perhaps the worst intriguer of all, his own son Oskar von Hindenburg.

Brüning's book is depressing because it shows that this chancellor of the Weimar Republic had all along the intent to restore the monarchy. He had already debated with Schleicher a year before he assumed office the idea that the return of the Hohenzollerns was to be the climax of sweeping reforms to be carried out perhaps under the emergency powers of Article 48 of the Constitution. On May 2, 1932, four weeks before he was dismissed, he prided himself in his last frank talk with Schleicher on the fact that he had reduced the influence of the Reichstag drastically and that the road to the constitutional monarchy was free. The next day he assured Hindenburg of the same. And he was even then considering

replacing his own moderate government soon by a rightist coalition headed by Carl Goerdeler!

Heinrich Brüning, the unselfish patriot, will retain our respect. His economic policy of radical deflation can be defended, even though its wisdom seems dubious in retrospect. The aims of his foreign policy make some sense; he shrewdly used the world depression to get rid of reparations. But his hostile attitude toward the republic will be a shock to many students of history. Now they must realize that the Weimar era did not end with Brüning's dismissal, but with the death of Stresemann.

FELIX E. HIRSCH

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ALBERT SPEER. *Inside the Third Reich: Memoirs*. Translated from the German by RICHARD and CLARA WINSTON. Introduction by EUGENE DAVIDSON. [New York:] Macmillan Company. 1970. Pp. xviii, 596. \$12.50.

The German original of this book, entitled *Erinnerungen*, appeared in 1969. The English version, except for a long passage (pp. 18–20) in which Speer berates himself for having joined the Nazi party in 1931, follows the original. One wonders why he thought it necessary thus to enlarge further upon his guilt. Already in his *Erinnerungen*, as well as at Nuremberg, he appropriated a far greater share of responsibility than the verdict called for. In a recent interview, published in *Playboy* (June 1971), Speer says that every confession makes him feel "freer." Maybe he also thinks that with an American audience self-accusation is the best self-defense. If so, he might do well to remember a lesson he learned while serving as Hitler's architect, namely that there can be too much of a good thing. Just as Hitler's giant edifices defeated their purpose by making the Führer "dwindle to an optical zero," so an exaggerated show of self-reproach tends to diminish the effect of sincerity it is supposed to convey.

The book consists of three parts. The first deals with Speer's background (affluent upper middle class), his start as an architect, and his early honeymoon with Hitler. Speer says he was as close a friend as Hitler ever had. The tenor of his book, despite the ambiguous story of how later he tried to kill his friend, bears him out. As for Hitler's reciprocating his feel-

ings, a contemporary has referred to Speer as "Hitler's unrequited love." Considering Hitler's forbearance toward Speer's last-minute acts of "treason," this also rings true. What tied these two unlike companions together was their interest in architecture. Much as Speer dissociates himself from the Führer's "architectural megalomania," one still senses a note of nostalgia for the days when he dreamt of becoming one of the world's greatest architects, serving a master builder who himself was not without architectural talent.

The balance of the book covers Speer's wartime activities, first as minister for armaments, and ultimately as director of virtually all German war production. In this role Speer describes himself as "the most important man after Hitler," and at one point even felt that he might become the Führer's successor. While this appraisal is exaggerated if checked against such works as Alan S. Milward's *The German Economy at War* (1965) and Willi A. Boelcke's *Deutschlands Rüstung im Zweiten Weltkrieg. Hitlers Konferenzen mit Albert Speer* (1969), Speer's contribution to Germany's war effort was still impressive and even decisive. His account adds some new dimensions, especially on his rivalry with such Nazi bigwigs as Himmler, Sauckel, and Bormann. The book's most dramatic part is the third, beginning with Himmler's attempt to do in his rival, and culminating in Speer's battle of wills with Hitler over the Führer's "scorched earth" policy. The final meeting of the two in the crumbling ruins of the *Reichskanzlei*, monument to their happier days, is sheer melodrama.

From what has been said, it should be clear that the central figure of Speer's story is Adolf Hitler. Of the many accounts by people who knew him, this is easily the best. Much of it is reminiscent of and confirms the revelations in *Hitler's Secret Conversations* (1953) and in A. Zoller, *Hitler Privat* (1949). Here again is the ordinary, dull, even kindly *Chef*, trying so hard to be human. But there are also glimpses of a less well known Hitler—constantly preoccupied with early death, worried over his father's illegitimacy and his own possible Jewishness, alternating between extremes of indolence and overwork; a remarkably self-controlled man, whose much advertised temper tantrums were merely put on for effect. Speer

does not solve the Hitler puzzle, but he does supply some of the missing pieces.

In reading so intensely personal a book, one cannot help wondering about the author's motive for writing it. Is it an honest effort of a good man to clear his conscience by telling all, regardless of where the blame may fall? Or is it a calculated whitewash, shifting the responsibility for the evils of nazism away from the German people and onto a gang of Nazi criminals, all of them dead except Speer, and he a victim of his environment, a product of our dehumanizing age of technology? Like Hitler, Speer remains a puzzle. When challenged, as in the interview in *Playboy*, he can say no more than what he has said already, and in very much the same words. His story has become an integral part of him. Yet the interviewer came away feeling that "a veil had been drawn" between Speer and the truth. This is an astute observation. It is this veil that impairs the usefulness of Speer's memoirs as a historical source.

HANS W. GATZKE
Yale University

GERHARD L. WEINBERG. *The Foreign Policy of Hitler's Germany: Diplomatic Revolution in Europe, 1933-36*. Chicago: University of Chicago Press. 1970. Pp. xi, 397. \$12.75.

The first of a projected two-volume work on Hitler's foreign policy, this book examines the initial four years of Nazi foreign relations. It is a comprehensive and detailed work based on a most extensive investigation of unpublished archival materials and published documentary collections. The list of secondary accounts used is the fullest I have yet seen. Footnotes are copious and most informative. The result of a decade's research and analysis, the text offers as much interpretation as fact, and both are well synthesized in a facilely written narrative.

Professor Weinberg begins with a review of Hitler's attitudes on racial superiority and on the domestic needs of the German people that in turn led him to insist on the acquisition of *Lebensraum*. The first step in this direction was to establish an all-pervasive dictatorship at home, the second was rearmament, and the third was war. War, for Hitler, was therefore "a deliberate prior choice," seen in terms of a series of conflicts rather than as a single, major conflagration. Acquisitions from each victor-

ious struggle would enrich Germany and further strengthen it for the next conflict. In 1933 war had to be postponed in favor of necessary domestic reconstruction, but since reconstruction was based on rearmament, the ultimate goal of aggressive expansion was served from the outset. Successful rearmament was in part due to favorable trade agreements and manipulation of German foreign exchange capital. In this regard the author considers German negotiations with the United States, China, Japan, and Southeastern Europe.

The book carefully examines German policy toward Poland, the Soviet Union, the major and minor states of Central and Southeastern Europe, the Far East, and the Western Hemisphere. The later sections offer a full coverage of the remilitarization of the Rhineland and an analysis of the power shift in favor of Germany by 1936. The guide for Nazi policy was promotion of bilateral accords, more easily broken than multinational ones, and use of a seeming elasticity in negotiations. What appeared to be peaceful solutions of problems existing between Germany and other states were in reality agreements that merely stalled for time. The goal was Austria, Czechoslovakia, and all of Eastern Europe under German hegemony.

Hitler took advantage of British and French incapacity to work in effective tandem regarding Ethiopia and the remilitarization of the Rhineland. He succeeded in splitting the two powers, destroying the League, and closing the gap between Germany and Italy. By 1936 the absence of any firm Anglo-French resolve had placed the initiative and advantage in German hands. The way to aggression lay open. It was now only a question of when the shooting war would begin.

Professor Weinberg's book fills a long-standing need for a widespread examination of Nazi foreign policy executed with scholarly exactitude. The second volume, which will complete the story, can only be awaited with eagerness.

GERARD E. SILBERSTEIN
University of Kentucky

BERNHARD UNCKEL. *Österreich und der Krimkrieg: Studien zur Politik der Donaumonarchie in den Jahren 1852-1856*. (Historische Studien, Number 410.) Lübeck: Matthiesen Verlag. 1969. Pp. 316. DM 40.

A new look at the diplomacy of the Crimean War has been due for a long time, particularly one that provides a better understanding of the policies of Austria, the power that found itself caught in the middle. This first book by the young German historian, Dr. Bernhard Unckel, is a significant attempt to meet this need. Basing his study upon research in the political and military archives in Vienna as well as on an impressive acquaintance with the literature, Unckel has produced a well-written account of Austria's Eastern policy from the death of Felix zu Schwarzenberg to the Paris Conference of 1856.

Disagreeing with those who see Austria as pursuing a *Schaukelpolitik*, Unckel believes that there was both continuity and consistency in Austrian policy. Emperor Francis Joseph and his foreign minister, Count Buol-Schauenstein, were continuing Schwarzenberg's program of securing Austria's predominance in Central Europe and extending her power to the south-east. The Near Eastern crisis of the mid-1850s, because it opened up tempting possibilities for expansion in the Balkans, caused Austria to develop this policy more fully. Vienna allied with the sea powers, hoping to be able to restrain their war aims, protect her own flank in Italy, and gain a foothold in the Danubian Principalities. But she was a poor ally. Her peaceful occupation of the Principalities eliminated her willingness to accept war with Russia; her financial crisis necessitated demobilization of her armies. The consequence was diplomatic isolation after 1856. At the bottom of this failure, according to Unckel, was an attempt to do too much. To preserve her position in Italy and Germany, to check Russia in the Near East and expand into that area herself, and to form a new alliance system with the "progressive" West, while maintaining her own nonnational absolutist system, was simply too much for Austria with her limited resources.

Given the overall quality of his work, one can only regret that Unckel chose to rely so heavily on previous accounts. His own research is particularly spotty in the diplomatic correspondence. His characterization of Austrian diplomacy in the 1850s as expansionist, moreover, is not convincing. Count Buol's continuous attempts to prevent and later to end the war do not support an interpretation of his policy as

opportunistic expansionism. Indeed, it seems grotesque to characterize Austria's essentially status-quo policy as expansionist when one compares it with the policies of the other powers, all of whom had ambitions for making significant changes in the European order and were willing to use the crisis in the Near East for this purpose. Such a comparison is beyond the scope of Unckel's work, yet a deeper insight into Austrian diplomacy will undoubtedly be found in its international context and, as Unckel himself suggests, in a closer examination of the relationship between Austria's foreign policy and her internal problems. A definitive answer to the questions raised by Austria's Crimean War diplomacy must await these inquiries. In the meantime Unckel has reopened the debate.

ROY A. AUSTENSEN

Illinois State University

GERARD E. SILBERSTEIN. *The Troubled Alliance: German-Austrian Relations, 1914 to 1917*. Lexington: University Press of Kentucky. 1970. Pp. xii, 366. \$12.50.

The title of this study, emanating from a doctoral dissertation at Harvard University, is misleading. The book does not cover the entirety of German-Austrian relations in the first part of World War I but only two aspects of "the troubled alliance." Almost three-quarters of the book are devoted to the Balkan diplomacy of the Central Powers and the rest to the relationship between the German and Austrian high commands and the eastern campaigns. Well documented, the study is based on thorough research in the Haus- Hof- und Staatsarchiv and Kriegsarchiv in Vienna and in the microfilm collections of the German Foreign Ministry Archives available in this country.

Professor Silberstein has provided much, perhaps too much, new detail in tracing the strenuous endeavors of Berlin and Vienna to win Turkey, Bulgaria, and Romania to their side, but he has found nothing new of significance. He does not dwell on the diplomatic contest in the Balkans between the Central Powers and the Entente and leaves out almost completely the activities of the latter. Instead, he expatiates upon the differences in the atti-

tudes and viewpoints of Berlin and Vienna and of their diplomatic agents regarding Balkan questions. His examination of the diplomacy of the Central Powers toward Turkey, Bulgaria, and Romania stops at the entry of each of these countries into the war. In contrast to part 2 on diplomacy, part 3 on the military side of the German-Austrian alliance is rather sketchy and revolves around the personality clash between the two chiefs of staff, Falkenhayn and Conrad von Hötzendorf.

The book is poorly organized and gives the impression of being a collection of loosely connected articles. For instance, it is not clear why so much space (most of chapter 2) in the introductory part is devoted to the Central Powers and the Greek question before the outbreak of the war since this question is not pursued further. Chapter 3 on military preparedness is not in its logical place. There is much repetition, and names are occasionally misspelled. The book has nine pages of selected bibliography and twelve pages of photographs of leading personalities.

ZYGMUNT J. GASIOROWSKI
University of Georgia

MARIO ASCHERI. *Un maestro del 'mos italicus': Gianfrancesco Sannazari della Ripa (1480 c.-1535)*. (Quaderni di "Studi Senesi," Number 22.) Milan: Dott. A. Giuffrè Editore. 1970. Pp. xi, 199. L. 2,600.

Students of Renaissance jurisprudence pay most attention to the innovators who applied the historical and philological methods of humanism to legal studies. There is ample justification for this, as such scholars as Alciato greatly changed Renaissance jurisprudence. On the other hand, the traditional legists have been practically ignored. The task of Ascheri is to rescue from obscurity one such figure.

Born and trained at Pavia, Sannazari was called to Avignon in 1518 to teach the *mos italicus*, that is, medieval method. This meant viewing Roman law as an unchanging formulation, carefully considering previous commentators, making careful distinctions, and finding agreement. Sannazari was an intelligent conservative; he formulated his own judgments, occasionally used humanistic methods, and found new practical applications for traditional precepts. The ability to make the law practical

won him the support of students and a higher salary than Alciato, who taught with and eventually attacked Sannazari. Recalled to Pavia in 1531, he was appointed to the chair of civil law at the very high salary of one thousand *scudi*, but he did not live long to enjoy it.

This monograph presents in well-documented form Sannazari's life and career and adds a very complete bibliography of his printed works and manuscripts. The monograph tends to follow Sannazari's life and career somewhat narrowly; it would have been better if the author had devoted more than nine pages to Sannazari's legal thought and had placed it better within the context of sixteenth-century jurisprudence. Yet the study is carefully done. Ascheri does not try to make Sannazari more than he was, a competent and esteemed traditionalist at a time when the bases of jurisprudence were changing. The author has performed a service in providing this study.

PAUL F. GRENDLER
University of Toronto

DONALD WEINSTEIN. *Savonarola and Florence: Prophecy and Patriotism in the Renaissance*. Princeton: Princeton University Press. 1970. Pp. viii, 399. \$13.50.

Savonarola has previously attracted scholars mainly because of his influence on Renaissance Florence; Professor Weinstein examines the influence of the city on the prophet. Florence's heritage of a special civic destiny, he shows, had long been suffused with religious and apocalyptic expectations. The universal prophet came preaching the scourge of God, and in the wake of events his message became one of hope, even certainty: a millennium would come, one cast in the mold of previous Florentine tradition; the city would be pure, wealthy, and imperial.

In an introduction distinguished by its literary style and intellectual acumen the author finds the earlier literature compromised in part by the politics of Pasquale Villari and Joseph Schnitzer. His attempt to free the prophet of Italian and Catholic apologetics and polemics has succeeded. Savonarola, he concludes, was more flexible than Roberto Ridolfi's ascetic, but also more fanatical than Schnitzer's reformer. He was neither the theorist of modern liberalism nor an opponent of middle-class

values. What then was he? For Weinstein, Savonarola evolved into a Florentine prophet, working within the city's traditions and bending to its contemporary necessities. The city, so to speak, invented its own prophet.

The author has come to his view of the man mainly through a masterful analysis of Savonarola's changing sociopolitical ideas, especially their "Florentinization." This analysis supersedes all earlier work. Weinstein's essays concerning the effect of the movement on the Laurentians and on Florentine religious thought after the prophet's death are very rich. The conclusion of the book goes beyond intellectual history to posit certain sociological and comparative theses that are challenging but undeveloped.

For the author the conquest of the prophet by the city furnishes the true drama of the period, and not what he calls Savonarola's brief conversion of the city. Savonarola's prophetic remoteness and Pauline imperiousness yield under Weinstein's artful pen to an at-times edgeless portrait of a man "domesticated" by tradition, "reconciled with the world of the possible," who came to "understand the political realities of the situation," a picture suspiciously like the myth of the young man chastened by the ordeal of power.

For my part, I remain unconvinced that Savonarola ever ended his "spiritual novitiate" when exposed to political necessity. In his premillennial stage the prophet had raged against homosexuality, gambling, ostentation, self-interest, and the like. His subsequent involvement in a millennial community only heightened his belief that men's vices were the bedrock of social disorder. To this end he fostered extragovernmental vice squads, abolished traditional festivals, and insisted on his right to preach. All this might be impolitic and incendiary, but without it there could be no Jerusalem. The author is of course well aware of this side of Savonarola, but it represents for him only a persistent "fanaticism" and "fantasy."

Thorough scholarship and fine writing make this book a major work in Italian religious and communal history. Weinstein has re-created the religious heritage within Florentine manifest destiny, and then placed a millennial movement and its prophet within that nexus. Sa-

vonarola and his devotees were indeed no aberrations. The prophet may have been somewhat more foreign and much more awful, his changing personality unobscured by our contemporary liberal proclivities, yet his utopian horizons were in the Florentine idiom. Weinstein has measured the impact of a political situation and tradition on a millennial impulse, and he has done it with great skill.

RICHARD C. TREXLER
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GIOVANNI MANGION. *Governo inglese, Risorgimento italiano ed opinione pubblica a Malta (1848-1851)*. Malta: n.p. 1970. Pp. 88.

PIERO BOLCHINI. *La Gran Bretagna e la formazione del mercato italiano (1861-1883)*. (Miscelanea Storica Ligure, New Series, Volume 1, Number 2.) [Genoa:] Università di Genova, Istituto di Storia Moderna e Contemporanea; distrib. by Casa Editrice La Nuova Italia, Florence. [1970.] Pp. 264.

Both of these essays may be described as studies of limited aspects of Britain's role in nineteenth-century Italian affairs, though dealing with very different subjects.

The British acquisition of Malta in 1815 was an expression of Britain's interest in the Mediterranean, the island cast in the role of an important naval base. Local sentiment was not opposed to British rule, but the liberal and unitary tendencies that flourished on the mainland had inevitable repercussions in Malta, a convenient refuge and center for the activities of Italian agitators, especially when Italian vicissitudes produced exiles, as was the case after the failure of the 1848 upheavals. The tradition of giving shelter to political refugees created something of a dilemma for Britain, concerned lest France reap advantage from the Italian revolutions, and desirous of preserving the European power equilibrium, hence loath to take any action that might antagonize friendly regimes.

This is the problem that confronted O'Ferrall, newly appointed governor of the island. The fact that he was Irish and Catholic made him particularly welcome at first, but the honeymoon was short-lived, for he was soon led to stop the influx of refugees from Sicily and the mainland. His refusal to allow the landing of

even some who came provided with passports issued by British consuls caused severe criticism from their local sympathizers. He was upheld in London, where the government was concerned in addition lest the unitary agitation raise the issue of the status of Malta. It should be pointed out, however, that the liberal agitation only affected a small section of the population, loyal in the main to its Catholic leaders, to whom the Roman Republic especially was understandably abhorrent. In 1851 O'Ferrall resigned his thankless task, though the influence of the fact that he pursued a conservative policy during a Whig administration seems questionable.

Bolchini's work is a typical doctoral dissertation, which traces in scholarly fashion the course of Anglo-Italian commercial and financial relations during the two decades following unification. The story begins in the heyday of the free-trade movement, of which the Cobden-Chevalier treaty marked a high point. England was interested in the Italian market and in Italy as a field of investment, and Italy adhered for a time to the liberal economic practice. But the Italian economy was very undeveloped, hence the usual effect of a demand for protection.

Moreover, Italy like others was affected by the broader fluctuations of the economic cycle, the crisis of the second half of the sixties, a brief revival, then the advent of the long depression. The heyday of free trade was past, and even in Britain voices were raised to question its merits. The end of the decade of the seventies saw the beginnings of a return to protectionism, and Italy followed suit. The change is closely connected with the revival of intense colonial activity in which Italy modestly shared. Also, from the British standpoint, competition was becoming keener, from Germany especially, and the beginnings of the British relative decline become perceptible.

RENÉ ALBRECHT-CARRIÉ
Rockford College

GIANNI AZZI. *Modena, 1859-1898: Condizioni economiche, sociali, politiche. Il movimento clericale dopo l'unificazione; le origini del socialismo modenese.* Modena: S.T.E.M.-Mucchi. 1970. Pp. 506. L. 4,000.

The capital of a small, authoritarian state until

1859, Modena then became a provincial city in a liberal nation. By 1898 though little larger than before, it had changed in other important ways. Still the center of its region, the stronghold of clericalism was shifting to the socialists; a quiet town of artisans and aristocracy was on the verge of becoming an important industrial center. The fascination of the subject is apparent, and the introduction to this study establishes that its author is aware of the possibilities. Here is a chance to explore the immediate, local effects of national unification and an opportunity to assess the intricate ties between town and countryside that were so important in the social structure of Italy. Modena was strongly affected by, yet not central to, every important political, economic, and social change of modern Italian history.

Azzi's interest in social history shows in the attention given to official statistics as well as to the parties of the left and to mutual aid societies. Despite its distinctive traditions, however, Modena emerges here as very similar to the rest of northern Italy, and that is the book's surprise. Although many remained loyal to the departed duke, although clerical strength was unusually high, although the economy was sorely strained throughout this period and unification in fact brought some tangible losses (the decline of the university, the elimination of government posts), Modena's politics appear to be much like the Romagna's and Tuscany's. That similarity, however, may result from a fault of method. Local history offers an opportunity to go behind crude categories of class and political party to study the personal and social connections by which a community operates. Instead, the discussion here is in terms traditionally used in national accounts; the groups involved are smaller but no more precisely defined.

A tantalizing reminder of how much could be learned about modern Italy from local histories, this book is evidence of how difficult the task is. Sources are hard to come by, and Azzi tends to treasure every find, preserving more than analyzing bits of private letters and public editorials. Much of the book in fact is organized around contentious newspapers, a procedure that conveys the tenor of political debate (the same in Modena as elsewhere; Modena's journalists appear not to have been

particularly original) but little of its social base. The long section on elections is among the most interesting, but its very length reflects the fact that elections more than most events leave a historical residue of statistical results and printed rhetoric. Local history, in short, easily leads the researcher into becoming a genial antiquarian, grateful for whatever turns up.

New data remain diffuse, and neither statistics nor social connections are probed systematically or with sophistication. Thus we learn little about economic growth or patterns of communication, little about local administration or the effects of national policy. Instead, what emerges is a lengthy, somewhat ill-digested account, containing many interesting moments and almost no conclusions. It reads like the memoirs of a long-lived, left-of-center but fair-minded local politician, written with an affection that establishes—as Gaetano Salvemini reassured the author—that Modena deserves a monographic study.

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I. I. LESHCHILOVSKAIA. *Illirizm: K istorii khorvatskogo natsional'nogo vrozozhdeniia* [Illyrism: A Contribution to the History of the Croat National Renaissance]. Akademiia Nauk SSSR, Institut Slavianovedeniia. Moscow: Izdatel'stvo "Nauka." 1968. Pp. 336.

The considerable attention that the Illyrian movement has received in Croatian historiography points to its importance in the history of the Croatian people. Illyrism, or the Croatian national renaissance of the 1830s and 1840s, was a cultural awakening that created the Croatian literary language and a political movement that laid the foundations of modern Croatian nationalism. As part of the Pan-Slav movement among the Slavs of the Habsburg Empire, it also aspired to establish cultural unity among the Yugoslavs. Much of the writing of the Croatian historians, however, has been marred by nationalistic bias, while some of the communist historians tend to see Illyrism as an essentially bourgeois and democratic movement. Leshchilovskaia's monograph, on the other hand, presents a balanced, scholarly ac-

count of the Illyrian movement. Indeed, it is the finest study on the subject to date.

Careful examination of the primary and secondary sources tends to support the author's conclusions concerning the nature of the Illyrian movement. She shows, for instance, that Illyrism was far from being a bourgeois movement as the Croatian communist historian Vaso Bogdanov maintains. It was a movement of diverse social composition in which the nobility was the dominant force with the intelligentsia from non-noble ranks playing a leading but not independent role. While the intelligentsia dominated the Illyrian press, the nobility alone fought for the realization of the Illyrian platform in the *županijas*, the *Sabor*, and the Joint Parliament of the Hungarian Kingdom. Moreover, if Ljudevit Gaj, the leading spirit from the intelligentsia behind the Croatian national awakening, shared the leadership of the movement with Janko Drašković, one of the most influential nobles, after 1843 this leadership passed altogether into the hands of the nobility.

In its political aspirations, as Leshchilovskaia demonstrates, Illyrism was a conservative movement. Confronted with the rising tide of Magyar nationalism, the Illyrians fought its main bearers, the Hungarian liberals led by Lajos Kossuth. Consequently, their political program, formulated by Drašković as early as 1832, was designed as a shield not only against the onrush of Magyarization but of Hungarian liberalism as well. As they accepted Gaj's national idea, the Illyrians tied the struggle for the national language together with the struggle to uphold and strengthen the historic rights of Croatia within the Hungarian Kingdom, that is, the feudal privileges of the nobility. Hence they opposed revision of the *Urbarium*, settlement of the Protestants in Croatia, or the extension of the vote in the *Sabor* to the peasant nobility no less vehemently than they resisted Hungarian and clamored for Croatian as the language of education and government in Croatia.

This is not to say that the Illyrian movement presented a unified ideology. In it Leshchilovskaia discerns many views and tendencies, both conservative and liberal. The author goes through considerable effort to construct an argument for the liberal platform. Formu-

lated by Gaj in 1841 and expressed in his frequently quoted motto, "God save the Hungarian constitution, the Croatian Kingdom, and the Illyrian nationality," Illyrian liberalism in action was confined to the issue of the national language. The language appeared as the symbol of the national unity and independence of the Triune Kingdom, and in that sense it was a political question. In that sense, too, the concept of national unity was a progressive one. But beyond this, Leshchilovskaia's evidence leads to the conclusion that Illyrian liberalism remained nebulous and had very little influence within the movement. It is clear, for example, that as the Magyar-Croatian conflict intensified, liberal no less than conservative Illyrians rallied more and more in defense of the historic privileges of Croatia. They sought support for their aspirations from the emperor in Vienna and in turn proved his loyal supporters. Indeed, it was on the advice from Vienna that the Popular party (as the Illyrian party was known from 1843) began to cooperate with the Hungarian conservatives in the same year (1846) when it rejected Ljudevit Vukotinić's proposal of a moderate liberal program. This unmistakable conservative orientation of the Illyrian liberals is further confirmed in 1847, when we find Gaj striving to influence the fate of Croatia not by calling for the liberalization of its politics but by seeking the title of nobility for himself. It was at this time that the few liberal Illyrians in the Popular party rejected its conservatism and its "unprincipled" leaders, and left it to found a party of their own. Thus Leshchilovskaia refutes unequivocally Bogdanov's claim that by 1847 the Popular party had evolved into an anti-Austrian, anti-Habsburg, democratic force, along with the view of the Croatian communist historian, M. Živančević, who sees its progressive elements as the "uncompromising revolutionaries." In fact, her analysis of Illyrism brings her closer to the thesis of the Croatian bourgeois historian, Fedro Šišić, who held that until 1848 the political platform of the Illyrian movement was based on Drašković's program.

This is neither a comprehensive nor a definitive study of the Illyrian movement. But it should be noted that Leshchilovskaia makes no such claims. For example, she concentrates on

the developments in Croatia, leaving out the Illyrian activities in the Joint Parliament. And she does not carry her study of Illyrism beyond 1847. Understandably, the Soviet author asserts her adherence to the basic principles of Marxism-Leninism, but her work is remarkably free of ideological preconceptions. Well written, this is history based on hard facts. As such, it is most welcome.

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EDGAR ANDERSON. *Tur Plīvoja Kurzemes Karogi* [There Waved the Flags of Courland]. [New York:] Grāmatu Draugs. 1970. Pp. 238.

This book is an addition to the historiography of the European expansion in Gambia during the seventeenth century. Although the English, the Dutch, and other European colonial and mercantile ventures are well known, the Courlander settlements in Gambia have been neglected or even studiously concealed by English and Dutch authorities. Edgar Anderson aspires to describe the expansion of the duchy of Courland in Gambia. He has a great predecessor, Otto Heinz Mattiesen, the German-Balt historian who concerned himself with the colonial policy of Courland in Gambia and the West Indies. Anderson's study continues Mattiesen's research, presenting some new facts. Anderson surveys not only purely colonial ventures, but emphasizes the social and economic aspects of the duchy of Courland. He is also very much concerned with the mentality of the various European adventurers in Gambia. Thus his work is completely free from the naiveté from which some authors suffer, especially those who deal with European colonial ventures.

The book contains a full critical apparatus and complete bibliography. The value of this study is enhanced by the fact that the author has used a wide variety of primary and secondary sources. Although some of the sources he uses are not very reliable, the shortcomings of this study resulting from the deficiencies of the sources cannot be charged to the author. His interpretations are essentially sound and based objectively on historical evidence. The Courlander colonial ventures are described in

a vivid narrative style. This work, written in Latvian, is concluded with a summary in English.

A few shortcomings of this study should be pointed out. Anderson asserts that some of the authors dealing with ventures of Courlanders in Gambia have made factual errors in several cases, yet he fails to indicate in his footnotes or in the text the sources that prove those errors. An intelligent reader, therefore, cannot check these statements. The author likewise tells the stories of the love affairs of the English king, Charles II, and other persons in boorish language, showing poor taste, to say the very least. Unfortunately, this scholarly study lacks an index.

All in all, in spite of some shortcomings, the book can be recommended for specialists and general readers alike.

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HANS-HEINRICH NOLTE. *Religiöse Toleranz in Russland, 1600–1725*. (Göttinger Bausteine zur Geschichtswissenschaft, Number 41.) Göttingen: Musterschmidt-Verlag. 1969. Pp. 216. Cloth DM 48, paper DM 38.

In his book Dr. Nolte provides a very careful and almost exhaustive study of the attitude of the Russian government and Church toward religions other than Eastern Orthodoxy during one of Russia's most crucial eras. Indeed, since the end of the sixteenth century the Russian state had absorbed territories with considerable pagan, Muslim, and Buddhist populations, while a large number of Protestants came to Muscovy to serve the state or to participate in Russian trade with the West. The author reminds us that in this period—the seventeenth and eighteenth centuries, which witnessed the Thirty Years' War, the dictatorship of Cromwell, and the revocation of the Edict of Nantes—the idea of religious tolerance suffered greatly in Western Europe. In Russia the attitude toward religions other than Orthodoxy was not determined by ideology, but in many cases the Russians were more tolerant toward the people of other confessions than their Western European contemporaries.

Governmental policies toward pagans, Muslims, and Protestants varied from decade to decade, but, on the whole, Russians did not

press their non-Orthodox subjects or foreign residents to become members of the State Church. Less tolerant were the Church and state toward the Catholics and Jews. Still, the most difficult position was that of the Old Believers, the dissidents who broke away from the main body of Russian Orthodoxy, creating their own autonomous denominations, and who were severely persecuted for their non-conformity.

Dr. Nolte's book is very systematically and clearly written and certainly deserves the attention of historians. I am looking forward to publication of the next part of his research, in which he promises to offer a study of the implications of Russia's religious policies toward non-Orthodox peoples.

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Istoriia mist i sil Ukraïns'koi RSR [History of the Cities and Villages of the Ukrainian SSR]. Kiev: Holovna Redaktsiia Ukraïns'koi Radians'koi Entsyklopedii Akademii Nauk URSR. M. A. SIROSHAN *et al.*, editors, *Kharkivs'ka Oblast'* [Kharkov Province], 1967, pp. 1001; I. T. BULANYI *et al.*, editors, *Poltavs'ka Oblast'* [Poltava Province], 1967, pp. 1027; I. F. PONOMARENKO *et al.*, editors, *Luhans'ka Oblast'* [Lugansk Province], 1968, pp. 939; V. I. MALANCHUK *et al.*, editors, *L'vivs'ka Oblast'* [Lvov Province], 1968, pp. 978; V. O. BOICHENKO *et al.*, editors, *Kyiv* [Kiev], 1968, pp. 585; L. V. GLADKA *et al.*, editors, *Odess'ka Oblast'* [Odessa Province], 1969, pp. 909; A. I. PASHCHENKO *et al.*, editors, *Dnipropetrovs'ka Oblast'* [Dnepropetrovsk Province], 1969, pp. 957; V. I. BELOUSOV *et al.*, editors, *Zakarpats'ka Oblast'* [Zakarpatskaya Province], 1969, pp. 785; V. M. KURYLO *et al.*, editors, *Chernivets'ka Oblast'* [Chernovtsy Province], 1969, pp. 703; I. S. KLIMASH *et al.*, editors, *Volyns'ka Oblast'* [Volyn Province], 1970, pp. 745; V. I. PETRYKIN *et al.*, editors, *Zaporizh'ka Oblast'* [Zaporozhe Province], 1970, pp. 764.

K. K. DUBINA *et al.*, editors. *Istoriia Ukrainskoi SSR* [History of the Ukrainian SSR]. In two volumes. (Akademiia Nauk Ukrainskoi SSR, Institut Istorii.) Kiev: Izdatel'stvo "Naukova Dumka." 1969. Pp. 832; 898.

ROGER PORTAL. *Russes et Ukrainiens*. (Questions d'histoire.) [Paris:] Flammarion. 1970. Pp. 140.

It is always hard to review an encyclopedia. When the work is both novel in subject and immense in scope, as is the *History of the Cities and Villages of the Ukrainian SSR*, one's

initial reaction is admiration tinged with awe. The impressiveness of the achievement is enhanced by the fact that the eleven volumes received so far—nearly half of the planned twenty-six—were completed within the space of three years, after about a decade of concentrated, devoted efforts by hundreds of Ukrainian scholars. Surely no other large country (in both area and population the Ukraine ranks among the larger national units of Europe) has a comparably detailed, up-to-date encyclopedia of local history.

The primary task of the reviewer, however, is not to praise, or even to summarize, but to indicate as precisely as possible both the value and the limitations of a work as a basis for further research. At the outset, therefore, one must plainly state that the *History* is not an analytic social history of the type so magnificently exemplified by recent products of the Sixth Section of the Ecole Pratique des Hautes Etudes (for example, Adeline Daumard, *La Bourgeoisie Parisienne de 1815 à 1848* [1963]; Louis Chevalier, *La Formation de la Population Parisienne au XIX^e Siècle* [1949]). Sociological and demographic analyses are making great strides in the USSR, but as yet the happy fusion of these disciplines with traditional history is not common; it would have been particularly appropriate in the present case. What might be accomplished by such cross-disciplinary work is suggested by Iu. V. Arutiunian's *Opyt Sotsiologicheskogo Izucheniia Sela* (1968), which, it happens, is devoted to one of the villages (Terpen'e, in Zaporizh'ka oblast) treated at length in the *History*. Unfortunately, neither the authors of the latter work nor Arutiunian seems to have been aware of the other's research. As a result, Arutiunian's perspective is restricted to the Soviet period, although his rich contemporary data might have been fruitfully related to social factors more remote in time. Conversely, the *History* would have been vastly enriched by use of data like those Arutiunian found in the USSR Ministry of Agriculture archives as well as by use of his more sophisticated conceptual framework.

The plan of the *History* made a synthetic examination of social factors virtually impossible. As the title indicates one volume is devoted to each *oblast* (province). Within each volume the organization is by *raions* (districts)

in alphabetical order—an arrangement that facilitates reference but that makes consecutive reading and even impressionistic comparison very difficult. Under the *raion*, towns and large villages receive extended treatment while smaller places (village soviet centers) are treated summarily. In all volumes except the one for Zaporizh'ka oblast, there is a brief summary of purely contemporary aspects (mainly statistical) of the *raion* as a whole, and each volume is preceded by a general historical treatment (about sixty pages) of the *oblast*. Evidently, however, none of these summary treatments is based on the more detailed local histories. Consequently (except as noted below), they add little to what is available in general histories and the recent *oblast Narodnoe Khoziaistvo* statistical volumes, though, taken together, the volumes of the *History* constitute a handy reference collection even at this level of utility. Similarly, the treatments of the large- and medium-sized cities (cities of "*oblast* subordination") are convenient, though they generally do not add much new information to what is scattered in more obscure Soviet publications. A major limitation in this connection is the fact that virtually uniform space (thirty-five to eighty pages) was allocated to the *oblast* center, whether it was a minor provincial city like Lutsk or Uzhgorod or a great, historic metropolis like Lvov or Kharkov.

Having bluntly stated these limitations, one can turn with a clear conscience to the enormous potential of the *History* as a source for systematic research. A significant but secondary value is provision of biographical data. Although there are no subject indexes, all volumes include excellent indexes of geographical locations and of personal names. While a cumulative index would be extremely useful, in most instances historians will, no doubt, be able to limit their search for information on particular individuals to a few *oblast* volumes. The kind of information available is indicated by the following examples (for the recent period only), which I happened upon: background details on Marshal G. I. Kulik, in the treatment of the village that still bears his name (*Poltavs'ka Oblast'*, pp. 826 ff); confirmation of the fact that in the spring of 1941 Władysław Gomułka officially became a member of the Soviet Communist party in Lvov (*L'vivs'ka Oblast'*, p. 76);

the assertion that in early 1945 Leonid I. Brezhnev, as chief political officer of the Soviet Eighteenth Army in Transcarpathia, played a major part in local "ideological" activity, which prepared for the transfer of the region from Czechoslovakia to the USSR (*Zakarpats'ka Oblast'*, p. 59).

Much more exciting are the possibilities for correlation analysis of social and demographic statistics. Although the presentation is erratic (nearly complete for *raions* in Poltava and some of the western border *oblasts*, rare elsewhere) there is enough statistical data on contemporary party and Komsomol membership to permit a far more detailed examination than has so far been possible of the relationship of this key variable to factors such as rural-urban population distribution, size of collective and state farms, number of schools, and, of course, geographical location.

Trend analysis—obviously more interesting to historians—is more difficult because of the frequent shifts in *raion* boundaries, and because the *History* presents statistics for more remote periods only for towns and major villages, and then only sporadically. Nevertheless, for any given *oblast* there is enough data on party membership for various periods since the October Revolution to enable one to establish reasonably probable trends that could be correlated to indicators of socioeconomic trends. The compilers of the local history sections were, of course, keenly interested in the rise of "Soviet power"; they therefore devote a great deal of space to both the underground Bolshevik organizations and to the local Communist parties in power. Much space is devoted to economic growth, especially during recent Five Year Plans. Agricultural collectivization in the East Ukraine is rather slightly treated, but collectivization in the territories acquired in 1939 and later is discussed at length; surprisingly, many discussions even give statistical details on the extent of "bourgeois nationalist" guerrilla opposition between 1945 and 1948. Communist guerrilla and underground activities during World War II are, of course, treated extensively, although the accounts usually add little to the abundant partisan literature already available.

In considering more remote periods, the local collaborators of the *History* were especially

concerned with the evolution of village class stratification, an especially complicated subject in the Ukraine. Usually the founding of each large settlement is described, and the origin of the settlers (particularly in the southern *oblasts* colonized in the late eighteenth or nineteenth centuries) indicated. Statistics on population stratification (Cossacks and other free settlers, state serfs, private serfs, and so on) are provided. Occasionally ethnic composition is also provided, though in general this question is neglected in the *History*. Additional information on social stratification and institutions (including, sometimes, the extent of repartitional tenure) is given for the emancipation period. Sizes of landholdings are indicated (by groups more or less in accord with the Soviet classification of "kulaks," "middle," and "poor" peasants) for various intervals between 1861 and 1929. The labor required to collate and analyze this data would be formidable, but the potential value is great indeed. Not only would such a work illuminate questions like the effects of emancipation upon agrarian social patterns, but it would permit one to examine in a detailed, cartographic manner the relation between traditional relations (such as landholding, or repartitional versus individual tenure) and peasant attitudes toward Ukrainian national identification, support of radical agrarian guerrilla movements (like Nestor Makhno's), and resistance to collectivization. In addition data on such historical factors might explain variations in contemporary statistical indicators such as education and party strength.

To sum up, the *History of Cities and Villages of the Ukrainian SSR* as it stands is highly useful as a reference work. The information it contains can be far more valuable if it is analyzed in detail by historians competent from a theoretical, statistical, and linguistic standpoint. It should be noted that the historian fluent in Russian will not require extensive study of Ukrainian to utilize the quantitative data; but certainly the availability of this immense work should impress upon the student of Russian and Soviet social history the indispensability of a minimal reading knowledge of Ukrainian. At the same time, it is fortunate that general guides to the subject are available in more familiar languages. The Russian version of the *History of the Ukrainian SSR* is a

revision of the two volumes that originally appeared in 1953 and 1958. In both cases Ukrainian as well as Russian versions were published. Contrary to what one might expect, revisions in the first volume (written before Stalin's death) were no more extensive than in the second volume. For example, the first volume of the new edition seems to have a slightly less "internationalist" tone but the second volume also contains less emphasis on such events as the circumstances leading to dissolution of the Polish Communist party (1938). Finally, for those desiring a clear, reliable, but skillfully compressed introduction to the complicated history of the Ukraine, Professor Roger Portal's new book is unsurpassed in any language.

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THEOPHIL HORNYKIEWICZ. *Ereignisse in der Ukraine, 1914-1922: Deren Bedeutung und historische Hintergründe*. Volume 3, *Ausrufung Paul Skoropadskyjs zum Hetman der Ukraine und dessen Regierungspolitik vom 30. April bis 25. Oktober 1918: Konsolidierung und Ukrainisierung des Staatsapparates—Festigung der Regierungsgewalt—Errichtung eines ukrainischen Schulsystems auf allen Ebenen (Volks- bis Hochschulen)—Bemühungen zur Schaffung einer regulären ukrainischen Armee—Ratifikation des Brester Friedensvertrages mit Deutschland, Bulgarien und der Türkei—Herstellung freundschaftlicher Beziehungen zu den Nachbarstaaten—Anknüpfung diplomatischer Beziehungen mit dem Ausland*; Volume 4, *Entstehung des Westukrainischen Staates und dessen Verteidigungskampf gegen Polen—Besetzung Ostgaliziens im Auftrage des Obersten Rates der Alliierten durch die Polen und deren anti-ukrainische Massnahmen—Räumung der Ukraine durch die österr.-ung. und deutschen Truppen—Politische Neuorientierung des Hetmans Skoropadskyj—Aufstand gegen den Hetman und sein Sturz durch das Ukrainische Direktorium—Politische und militärische Lage in der Ukraine nach der Machtübernahme durch das Ukrainische Direktorium—Vereinigung der Ost- und Westukraine und deren gemeinsamer Kampf gegen die sowjetische Invasion in der Ukraine—Warschauer Konvention Petluras mit Polen—Besetzung der Ukraine durch die Sowjets—Aufstandsbewegung gegen das Sowjetregime in der Ukraine*. (Publications of the W. K. Lypynsky East European Research Institute, Series I-IV, Volumes 3 and 4.) Philadelphia: the Institute. 1968; 1969. Pp. lx, 704; xlviii, 421. \$16.00 each.

PETER BOROWSKY. *Deutsche Ukrainepolitik 1918 unter besonderer Berücksichtigung der Wirtschaftsfragen*. (Historische Studien, Number 416.) Lübeck: Matthiesen Verlag. 1970. Pp. 316. DM 44.

SERHII MAZLAH and VASYL' SHAKHRAI. *On the Current Situation in the Ukraine*. Edited by PETER J. POTICHNYJ. Introduction by MICHAEL M. LUTHER. Ann Arbor: University of Michigan Press. 1970. Pp. xxxiv, 220. \$8.95.

MATTHEW STACHIW and JAROSLAW SZTENDERA. *Western Ukraine: At the Turning Point of Europe's History, 1918-1923*. Volume 1. Edited by JOAN L. STACHIW. (Shevchenko Scientific Society, Ukrainian Studies, English Section Volume 5.) Scranton, Pa.: Ukrainian Scientific Historical Library. 1969. Pp. 324. Cloth \$5.50, paper \$4.50.

The four works discussed in this review possess a common denominator: they deal with aspects of the revolutionary and civil war era in the Ukraine. *Ereignisse in der Ukraine 1914-1922*, volumes 3 and 4, edited by the late Theophil Hornykiewicz, brings to a conclusion a publication of documents from the Österreichisches Staatsarchiv in Vienna. The third volume is of outstanding interest. It covers the period of the German and Austro-Hungarian occupation of the Ukraine in 1918. The reporting of the Austro-Hungarian diplomatic and military representatives was truly exhaustive and treated not only the relations of the occupying forces with the Ukrainian government, but also virtually all facets of Ukrainian life. A topical ordering of the documents and good tables of contents and indexes make the work very useful. In all, *Ereignisse in der Ukraine* is the most valuable collection of documents on the history of the Ukrainian revolution to appear to date, and it will be indispensable to all students of that era.

A welcome supplement to this publication of Austrian documents is Peter Borowsky's monograph, *Deutsche Ukrainepolitik 1918*, based primarily on a rich store of untapped German documents. The author is a student of Fritz Fischer, and he conceived his study in terms of a re-examination of Fischer's much-debated thesis on Germany's aims in World War I. Borowsky's main point is that, contrary to the views of some recent German historians, Brest-Litovsk was more than a mere "bread peace," and that the occupation policy in the

Ukraine went far beyond the requirements of a temporary military contingency. The Ukraine was to become a permanent German dependency and serve also as a base for the political and economic penetration of other sections of the former Russian empire. This was to be a compensation for the loss of colonies and overseas trade. Frictions between Germany's military leaders and civilian statesmen in regard to the Ukrainian policy were of a tactical nature only. Ludendorff stood for an old-fashioned type of imperialism that thought in terms of territorial annexations and naked military coercion. The diplomats and economic planners represented a more flexible and subtle, modern type of imperialism that preferred the methods of indirect control. Borowsky rejects the often repeated view that Germany's Ukrainian policy in 1918 was a failure. It is true that because of the shortness of time and various technical difficulties the Germans were unable to reap in the Ukraine all the expected economic benefits. But politically they remained in a dominant position until the late fall of 1918. The end of Germany's Ukrainian venture was due not to any local causes but solely to the German collapse in the West. Peter Borowsky's book is to be commended as a model of sober, scrupulous scholarship. It should be noticed, however, that the author did not intend to produce a history of the Ukrainian revolution and independence movement, and his treatment of internal Ukrainian developments is only incidental.

On the Current Situation in the Ukraine is the translation of a book-length pamphlet originally published in early 1919. The authors, Serhii Mazlakh and Vasyl' Shakhrai, were prominent Ukrainian Communists. At the same time they were convinced that the drift of the Ukrainian revolution was toward national statehood. Therefore they advocated the establishment of a fully independent Ukrainian Soviet Republic and of a Ukrainian Communist party separate from the Russian party. The most significant part of the book is a detailed and drastic exposure of the contradiction between Bolshevik slogans of national self-determination and actual Bolshevik practices in the Ukraine. The work, which is distinguished by intellectual brilliance and polemical vigor, culminates in a direct personal challenge ad-

ressed to Lenin. *On the Current Situation in the Ukraine* is probably the first formulation of a "national communist" standpoint in Marxist literature. Thus it illuminates not only the specific Ukrainian constellation of 1919 but also the broader, perennial issue of national tensions within the Communist camp. The text has been competently translated and edited by Peter J. Potichnyj.

The subject of *Western Ukraine at the Turning Point of Europe's History, 1918-1923*, volume 1, by Matthew Stachiw and Jaroslaw Sztendera, is the emergence of a Ukrainian state in the former Austrian province of Galicia in October-November 1918 and the ensuing Polish-Ukrainian war for the possession of that territory. The narrative of this first volume is carried through May 1919; a second volume is to follow. American students of modern Ukrainian history have directed their researches to the east-central (Russian) Ukraine, while neglecting the western section. But the latter, though much smaller in size and population, was the chief stronghold of Ukrainian nationalism. No understanding of modern Ukrainian history can be adequate without taking into account the Galician contribution. This gap in English-language historical literature might have been filled by Stachiw and Sztendera, but unfortunately their work is marred by serious shortcomings. Some valuable factual information may be found in the book—for instance, on the legislative and administrative activities of the West Ukrainian People's Republic and the attempts of the Allied Powers to settle the Polish-Ukrainian conflict. On the other hand, the reader has to cope with a clumsy organization of the material, a wooden style, numerous typographical errors, misspelled names, blurred illustrations and maps, and a method of citation unacceptable in a scholarly work. Even more unpleasant are the authors' cantankerous tone and their inability to approach the subject with emotional detachment. A history of the western Ukraine still remains to be written.

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A. K. BABKO et al., editors. *Istoriia Akademii Nauk Ukrain'skoi RSR* [The History of the Academy of Sciences of the Ukrainian SSR]. In two volumes. (Akademiia Nauk Ukrain'skoi

RSR.) Kiev: Holovna Redaktziia Ukrains'koi Radians'koi Entsyklopedii Akademii Nauk URSR. 1967. Pp. 834; 725.

The date given for the founding of the Ukrainian Academy in this history is February 12, 1919, after the establishment of Soviet rule. Nothing is said about the fact that the foundations of the academy were actually laid by the pro-German government of General Pavel Skoropadski on November 14, 1918. Between 1921 and 1937 the name of the academy changed three times. Until 1921 it was called the Ukrainian Academy of Sciences. Since 1937 it has been known as the Academy of Sciences of the Ukrainian Soviet Socialist Republic. The *History* traces the evolution of the academy during the NEP and each of the five-year plans, under different presidents (Vernadskii, Zabolotnii, Bogomolets', Palladin, Paton), and at every stage of socialist development in the Soviet Union before and after the Second World War. Almost every change in the Soviet Union's domestic and foreign policy had its repercussions in the academy.

In 1939, after the incorporation of "Western Ukraine" into the Soviet Socialist Ukraine, several men of learning from Lwów were brought into the academy, which at the same time acquired new research units. The German invasion in the Second World War made it necessary to transfer the academy's personnel and much of the equipment, research material, and some valuable manuscripts (those of Gogol, Shevchenko, Frank, Gorky, and Turgenev), from Kiev to Ufa. The academy geared its activities henceforth to the needs of the war effort and apparently rendered a substantial service to the Soviet Union.

In 1944 the academy returned to Kiev, and its work was redirected to peacetime investigation connected with the country's economic development. During the ensuing years the academy's organization steadily expanded. In 1965 it had under its umbrella sixty scientific institutions, including research institutes, observatories, botanical gardens, and libraries. The academy's library held in excess of five million volumes. Its membership consisted of 92 academicians, 127 affiliated members, and a working staff of about 30,000 scientists and administrators. The *History* tells us how many of these persons were members of the Communist party

in 1958 and in 1967 and about the role of the Communist party in the development of the academy.

The Ukrainian suspicion of the Russians and Russification has been a permanent source of friction between the Ukrainian scientific world and Moscow. This largely explains the controversy over the All-Union planning of sciences and the resistance in the Ukraine to Moscow's control of the Ukrainian Academy of Sciences. The history of the academy has been plagued by this suspicion of the Russians and by a relentless campaign against non-Marxist scientists who deviated from the official line or for other reasons fell into Stalin's disfavor.

Although references are made to some of the numerous purges of the academicians, the *History* provides no meaningful discussion of them except for complaints against the presence of bourgeois nationalistic ideology and the damaging influence of Mikhaïlo S. Hrushevs'kii (1866-1934), the author of the well-known, ten-volume *Istoriia Ukrainy-Rusy* (1903-36) on historical, legal, and literary research in the Ukraine. Time and time again the academy or one or more of its institutes or members was charged with anti-Soviet and reactionary positions. In the course of the 1930s many persons were liquidated on the grounds that they belonged to a nationalistic organization that allegedly sought to detach the Ukraine from the Soviet Union. For this and other reasons biographies of some erstwhile members of the academy are not given. Yet, interestingly, Stepan Prokopovich Timoshenko (born in 1878), a founding member of the academy and for many years professor at Stanford University, is spoken of with respect and given a short biography.

Despite various shortcomings the *History* has real value as a chronicle of the academy's accomplishments and developments that have made it a great center of learning. For this reason it will serve as the principal reference work on the Academy of Sciences of the Ukrainian SSR.

WAYNE S. VUCINICH
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O. L. VAINSHTEIN. *Istoriia sovetskoi medievistiki, 1917-1966* [The History of Soviet Medieval Studies, 1917-1966]. (Akademiia Nauk SSSR, Institut Istorii, Leningradskoe Otdelenie.) Len-

ingrad: Izdatel'stvo "Nauka," Leningradskoe Otdelenie. 1968. Pp. 422.

The medievalist, as well as the student of modern Soviet Russia, will be interested in consulting Vainshtein's impressive prosopographical and bibliographical review of Russian medieval scholarship since the Revolution. Before and after 1917 innumerable Russian scholars have greatly contributed to the knowledge of the Middle Ages, west and east. Unfortunately, the language barrier keeps much of their achievements from being used by their European or American colleagues. To those Western medievalists who know Russian, especially the specialists in Byzantine and Slavic history, this book can serve as a useful introduction. It also uncovers, with unusual frankness, the facts that the first two decades of communism in Russia have almost interrupted the tradition of medieval scholarship and that the recovery, observable since World War II, is not so much a new Marxist era of scholarship, as the author affirms in an introductory chapter (pp. 7-26), but a partial return to and respect for the old "bourgeois" methods and problematics.

"After the establishment of Soviet power," Vainshtein writes, medieval studies "were excluded from the programs in institutions of higher learning and remained, for many years, outside of the mainstream of Soviet historiography" (p. 70). The author mentions many names of "bourgeois" scholars who went into emigration and recognizes that those who remained in Russia were seriously handicapped in their scholarly work. However, he refers to their works—published in the USSR or abroad—with respect and sometimes with admiration. By 1934-36, when historical studies were reorganized and new opportunities arose, "the number of medievalists, who had adopted Marxist positions, was negligible" (p. 92), and since only the latter were permitted to work and publish, the results were admittedly modest.

"Marxist positions" in medieval studies are defined by the author through the usual quotations from Marx, Engels, and Lenin, emphasizing social and economic factors in the development of medieval society and, particularly, of feudalism. Admittedly, much of the remarkable development of Soviet medieval studies in the postwar period is devoted to economic history,

interpreted in Marxist categories, but a Western historian will rather see this fact as a limitation; and most Soviet historians will hardly agree that "Soviet medieval studies are a product of the Great Socialist Revolution, which created the Soviet State and Soviet society" (p. 412). They would rather think that medieval studies survived the Revolution and were developed, especially in the Byzantine and Slavic fields, in spite of the ideological limitations to which publications were and still are subjected—although to a lesser degree than under Stalin (the author recognizes that in the late forties the text of scholarly publications could be modified by censors without the author's knowledge, p. 194, n. 30).

Thus, Vainshtein's book, if read carefully, reveals some of the tragedies that characterize all free intellectual pursuits in Soviet Russia, but it also witnesses the remarkable achievements of many scholars. It contains information about the personal fate of several well-known medievalists in the postrevolutionary years and refers to innumerable little-known publications. Under the cover of formal Marxist orthodoxy—which is often mainly verbal—their disciples and successors achieve an increasing convergence in historical methodology between modern Soviet and the old Russian historiographical schools. The latter is not suppressed any more but only characterized as "bourgeois" and then freely used as source and example.

JOHN MEYENDORFF

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LOWELL TILLET. *The Great Friendship: Soviet Historians on the Non-Russian Nationalities*. Chapel Hill: University of North Carolina Press. 1969. Pp. x, 468. \$12.50.

Fifteen years of dedication and loyalty to research in addition to Professor Tillett's total command of the subject are the outstanding features of the work under review. American East European historiography, while making progress in Russian and Soviet areas during the last two decades, almost completely neglected at the same time the non-Russian aspect of the USSR. Professor Tillett's work represents, therefore, a remarkable breakthrough, and, apart from Dmytro Doroshenko's *Survey of Ukrain-*

ian Historiography (1957) and a few articles in professional journals, it is a pioneering contribution.

Professor Tillett's novel approach—he chooses one central theme and describes the principal developments in the dialogue between party and scholars with the resulting changes—requires expert knowledge on the part of the author and the reader. Dr. Tillett undoubtedly succeeds in a superior fashion, and we are given an insight into Soviet historiography through a little known dimension. His revaluations and the conclusions, reached after obviously careful analyses, are unmistakable and not easily rebutted.

Part 1 describes the making of the myth of friendship among Soviet nationalities; part 2, arranged topically, analyzes the substance of the myth. The following topics were chosen to typify Soviet historiography dealing with the non-Russian peoples: the deepening of historical ties, Russian military aid, the processes of Russian expansion, the progressive consequences of annexation, and the cultural scene. One would like to see included the economic, educational, and ideological aspects, which are only marginally treated; yet it is doubtful that the author's findings would have to undergo any significant changes—for example, his conclusion that “the new historical myths [Soviets] differ from the old ones [Mussolini's and Hitler's] both in kind and degree. Never before the proponents of myths made such claims for the ‘scientific’ bases of their theories.”

Laboring diligently through hundreds of Soviet works, articles, and official documents, Dr. Tillett exposes the Soviet claims that Leninist nationality policy has created something entirely new in history—a multinational society without national hostilities—and that the party's nationality policy created the friendship among the peoples. In fact, Dr. Tillett asserts, “It is too early to determine whether the new Soviet history . . . will make the desired contribution to the reduction of nationalist tensions among the peoples of Soviet family. . . . The party appears to be pleased with the accomplishment of Soviet historians, as is indicated by the recent cordial relations between party and scholarly forces, and the lack of controversy.” Exactly this “lack of controversy”

among Soviet historians, and between them and the party, reduces Soviet historiography to an auxiliary discipline, and the case tested on the problem of non-Russian nationalities eliminates any doubts. Silence and obedience, as in the darkest tsarist times, continue unabated.

A thirty-four-page bibliography, an eight-page index, a glossary of historical terms, and a map of nationalities enhance the reference value of this book. Ample footnotes at the bottom of the page and the excellent typography are a credit to the publisher.

It is hoped that this outstanding contribution by Professor Tillett will stimulate further study and research in the history of Ukraine, the Baltic republics, Caucasia, and the Central Asiatic area among American scholars and students who cannot afford to be without this work on their desks, as university libraries cannot afford to be without it on their shelves. Professor Tillett opens new vistas with merit and maturity; hence he deserves our tribute.

STEPHAN M. HORAK

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WILLIAM ZIMMERMAN. *Soviet Perspectives on International Relations, 1956–1967*. (Sponsored by the Institute of War and Peace Studies and Russian Institute, Studies of the Russian Institute, Columbia University.) Princeton: Princeton University Press. 1969. Pp. 336. \$9.50.

In this volume of careful scholarship Mr. Zimmerman tries to marry a social scientific conceptualization of the topic to historical data and therefore to historical explanation. Each of these methodological aspects of the study limits the other. But within the limits Mr. Zimmerman does exceptionally well.

The historical literature of Soviet politics abounds with decisively opinionated, eclectic tracts that cumulatively balance one another off without affording much middle ground, hence straining the credulity of any reader unwilling to accept either set of polar assumptions. It is the equally sorrowful fate of social scientists to pose judicious questions for which the necessary aggregate data are largely unavailable, letting the reader slowly but relentlessly down into a dissatisfying maze of unsustainable conjectural hypotheses. Mr. Zimmerman avoids both pitfalls. His conceptualization helps

him resist eclecticism, and his history yields convincing interpretations. His study emerges as the best monograph available to explain how the USSR's international relations specialists, rather than official decision makers, looked at the outside world and particularly the United States between 1956 and 1967.

An introductory chapter orients us to the growth of Soviet interest in the study of international relations and the evolution of the Soviet perspective during the Khrushchev and immediate post-Khrushchev years. Zimmerman introduces here the three persisting themes in the Western literature that he will take as the focus of his substantive inquiry: the relevance of Marxist-Leninist ideology, continuity versus change, and spontaneity versus consciousness in Soviet foreign policy. He introduces us to the problems of inference he faced and to his sources: the leading Soviet journals of international relations, books by academic specialists, major official documents and speeches—augmented by interviews and Soviet dissertations obtained during a semester in the USSR in 1966.

Subsequent chapters address the emergence of international relations as a systematic discipline in the post-Stalin, nuclear-threatened environment, then the evolution of Soviet views of the structure of the international system, its actors, their hierarchy, and the distribution of power among them. Soviet perceptions of United States policy are traced, then attitudes toward the balance of power as description and prescription. The concluding chapter offers some convincing assessments, notably of the present leadership's resignation to failure of the Khrushchevian effort to resynthesize Marxist-Leninist theory with the practice imposed by growing interest in stability of an international equilibrium. The volume ends with one of the best available summaries of the diminished but still significant roles of Marxism-Leninism among Khrushchev's cautious successors.

Two reservations should be noted briefly. The line between inferences we may draw about academics and staff specialists on the one hand and decision makers on the other is not always clearly drawn, nor is the relationship between the two. And, from a social scientist's perspective, some use of what aggregate data

are available would complement the author's historical tracing of the themes of Soviet commentators. On balance, however, Mr. Zimmerman has managed a challenging subject effectively. His book adds appreciably to our understanding of Soviet outlooks in evolution; it is also a provocative example of the compatibility of historical explanation and social science.

DAVID D. FINLEY

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ASIA AND THE EAST

MA HUAN. *Ying-yai sheng-lan: 'The Overall Survey of the Ocean's Shores' [1453]*. Translated from the Chinese text edited by FENG CH'ENG-CHÜN, with introduction, notes and appendices by J. V. G. MILLS. (Hakluyt Society, Extra Series, Number 42.) New York: Cambridge University Press for the Society. 1970. Pp. xix, 393. \$35.00.

The seven voyages commanded by the eunuch admiral Cheng Ho from 1405 to 1433 constituted a startlingly impressive if short-lived display of Chinese naval power. What made them possible was the Yung-lo emperor—the usurper who sponsored them—and a socially marginal amalgam of military men, eunuchs, and Chinese Muslims who commanded and directed them. All across maritime Asia, from Indochina to the Persian Gulf, the main fleet (forty-eight ships and thirty thousand men on the third voyage) enrolled local rulers as Ming vassals, elevated obscure chieftains to tributary “kings” (*wang*), quelled pirates, punished the unsubmissive, and engaged in trade. Detachments from the main fleet reached as far as Jidda in Arabia and Malindi on the coast of modern Kenya. Even though as a technological and logistic achievement the voyages were unprecedented in China's history, accounts of them were deliberately ignored in the gentry-dominated literary tradition. Their rediscovery and revaluation had to await modern times.

Ma Huan, a Chinese Muslim from K'uai-chi in Chekiang province, accompanied three of the voyages as an official translator and composed one of the three extant travel records of the Cheng Ho expeditions. His notes are not, as might be expected, in diary form but are synthesized into a brief descriptive catalog of twenty countries (*kuo*) of South and Southeast Asia, including Mecca (T'ien-fang kuo), all of which he seems to have visited personally.

J. V. G. Mills's study and translation of Ma Huan is addressed primarily to persons interested in the history of geography and material culture. Ma Huan was much concerned with listing curious customs and strange products, and Mills's annotations serve as a good guide to the large body of earlier scholarship on these matters. In addition Mills has provided a lengthy introduction and eight technical appendixes, the latter mainly devoted to aspects of traditional cartography and the history of place names. The longest appendix is devoted to a discussion of the so-called Mao K'un map of maritime Asia in the seventeenth-century *Wu-pei chih* (Gazetteer of Military Defense).

The format of this book and the several maps included in it are handsomely executed. Mills's translation of Ma Huan's descriptive material appears reliable on the whole. Readers should be warned, however, that where the text becomes more literary, notably in the forewords, the translation tends to be awkward and in a few crucial places quite wrong. This is regrettable, for it is the prefatory material that touches upon such significant but elusive problems as the motivations behind the voyages, or the cultural self-image and social aspirations of a marginal Chinese who at least nominally identified himself with Islam.

JOHN W. DARDESS
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FELICIA GRESSITT BOCK, translated with introduction and notes by. *Engi-shiki: Procedures of the Engi Era*. Books I-V. Tokyo: Sophia University. 1970. Pp. x, 216. \$12.00.

Engishiki is a large, functionally arranged collection of detailed regulations for the operation of the system of government instituted by the Japanese court in the early eighth century under the influence of Chinese models and practice. The court's basic penal and administrative code (*ritsuryō*) required not only frequent amendment to meet changing conditions during the two hundred-odd years of its effective life, but also detailed supplementation in order to give practical effect to its articles, which tended to be limited to broad statements of principles and aims, especially in the administrative sections. The accumulating legislative amendments (*kyaku*) and supplementary regulations (*shiki*, "procedures," as Mrs. Bock calls

them) were brought together in comprehensive collections on three occasions, the last in the tenth century, when the final stage in the formal development of the court administrative structure was reached with the compilation of *Engishiki* (presented to the throne in 928). Although the two earlier collections of supplementary regulations, and all three collections of legislative amendments, survive only in fragmentary form, *Engishiki* remains virtually intact, its great bulk—over one thousand demy octavo pages in a modern printing—constituting one of the richest sources available for the history of the Nara and early Heian periods. Its contents, arranged for the most part according to the central government offices to which the regulations applied, cover a wide spectrum of religious, social, economic, and political life—often creating by their very detail an almost impenetrable thicket of historical and philological problems, but also preserving, as do no other extant documents, contemporary concepts of how the practical, day-to-day administration of state affairs was to be conducted.

Mrs. Bock's monograph represents the first serious attempt to introduce *Engishiki* to English-reading scholars. It consists primarily of a translation of the first five chapters, or about one-tenth of the entire text. These chapters, dealing mainly with the more important Shinto festivals and rites, devote perhaps as much as three-quarters of their total content to simple lists of offerings and other supplies required for specific religious observances. The translation is preceded by four short introductory chapters, which treat the development of law in Japan, the Jingikan (the court office in charge of Shinto matters), the cult of the Sun Goddess, and subjects relating to the *Engishiki* text and the content of the translation.

Although *Engishiki* is strewn with difficult problems of understanding and interpretation, Japanese scholars have yet to produce an adequate commentary, and one can only be impressed by Mrs. Bock's courage in undertaking even a partial translation of such a text. Inevitably, perhaps, her understanding of its meaning and her treatment of its many problems are not always convincing, but the translation nevertheless performs a useful service by acquainting Western scholars with the general nature of *Engishiki*'s historical riches. Mrs.

Bock's work should stimulate interest and, one hopes, further progress in this important, though extraordinarily demanding, field.

WILLIAM MCCULLOUGH

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JOHN MACGREGOR. *Tibet: A Chronicle of Exploration*. New York: Praeger Publishers. 1970. Pp. 373. \$13.95.

This book is an eclectic synthesis of various accounts of Westerners who have traveled in Tibet over the centuries. The prologue deals with missionaries attracted to Central Asia by the legend of the great king of the Christians called Prester John and concludes with a summary of the journey of Odorico of Pordenone to Tibet in the early fourteenth century.

The bulk of the book is divided into three parts. "The Missionaries" treats the Jesuits and Capuchins who traveled in Tibet from the late sixteenth century to the early eighteenth century, including accounts of Ippolito Desideri's stay at Lhasa, the Dzungar invasion of 1717, and the fate of the Capuchin mission.

"The Merchants" surveys the unsuccessful attempts made by the British in India to establish trade relations with Tibet through the visits of George Bogle and Samuel Turner to the Panchen Lama at Tashilhunpo monastery near Shigatse. Following the war with Nepal in 1788-92, Tibet was closed to the British and remained so until the beginning of the twentieth century. This part is followed by an "Entr'acte" touching on the visit of Thomas Manning to Lhasa in 1811, and the visit of the Lazarist priests, Evarist Huc and Joseph Gabet, in 1846.

"The Imperialists" reviews the Tibetan aspects of the "great game" played by Russia and Great Britain for control of central Asia, a game that finally caused Lord Curzon to send the commission headed by Colonel Francis Younghusband into Tibet, where long-sought trade agreements were concluded in 1904. The epilogue summarizes events after the British mission was withdrawn.

According to the dust jacket the author of this book is a State Department diplomat using the pseudonym of "John MacGregor"; thus there is no way to assess his personal qualifications. From the frequency of misspelled Tibetan words, however, it appears that the author does

not know the language. This could be overlooked if the book were relatively free of other mistakes, but unfortunately wrong dates, incorrect data, and typographical errors occur with disappointing regularity. For example, MacGregor states that the sixth Dalai Lama was murdered near Li-tang in 1707, yet Tibetan and Chinese sources state he died of illness near Kun-dga'-nor, a small lake near Lake Kokonor, in November 1706. MacGregor has drawn upon a wide range of materials in preparing this synthesis, and it would have been a valuable contribution to the field if it had been carefully edited by someone more knowledgeable about Tibet and its culture.

TURRELL V. WYLIE

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AMERICAS

STANLEY COBEN and LORMAN RATNER, edited, with an introduction, by. *The Development of an American Culture*. Englewood Cliffs, N. J.: Prentice-Hall. 1970. Pp. iv, 235. \$4.25.

This collection of essays is unusual for books of its type. Almost inevitably such collections are uneven in theme and quality. *The Development of an American Culture* is, to be sure, not exempted from the rule. Witness, for example, Alden Vaughan's disappointingly traditional survey of colonial America, which is only peripherally related to the development of the national culture and character. Robert Middlekauff's symbol analysis of revolutionary ritual lacks depth and density. It fails to explore the rise of community awareness, neglects the rich literature suggestive of a proliferating national spirit, and unaccountably ignores Richard Merritt's pioneering and instructive *Symbols of American Community* (1966). But even in these opening essays, as throughout the collection, the writing is irresistibly intelligent, easy to read, and not overburdened by sociological or behavioral analyses. Moreover, most of the contributors are judicious, intellectually alert, and not easily imposed upon—qualities essential to those engaged in the inextricably enmeshed dual chore of synthesizing and dismantling earlier interpretations.

The best of these essays have the wind of originality whistling through their pages. They are less attentive to Matthew Arnold's tradi-

tional high-culture approach and more to social history, which, after all, has increasingly engaged students of our cultural past, and they are profoundly aware of the reciprocity of history and culture as well as of the enormous complexities involved in untangling pluralistic, subgroup, subculture manifestations, even within the limited chronological period staked out for them. John Ward, for example, explores Jacksonian political culture, extrapolating from political speeches and essays of the time, and emphasizes those antebellum sentiments that celebrate the capacity of a virtuous people, the rough egalitarianism that denied power to any individual or group of individuals, and the necessity of negative government for preindustrial America. Ironically, of course, Old Hickory strengthened the executive office in the course of his attempt to "strip government of power." Compounding the irony, the "ideal of a simple, uncomplicated society . . . found its most persuasive political voice" at a singularly unpropitious moment, when a "take-off" would transform pastoral America into a society committed to wealth and power.

Carl Degler, writing of "Two Cultures and the Civil War," also sees the paradoxes and divergences that underline sociocultural history. Examining slavery's cultural values, he explains how these helped to create a distinctive regional culture—provincial, defensive, fundamentalist, antiforeign, antiurban. Nothing new, to be sure, but a story told with the grace and obvious mastery of monographic materials that one has come to expect of Professor Degler. His contribution, however, has the defects of its apparent virtues. For it reminds us of the difficulties that inhere in a hurried examination, of how acquaintanceship with any period must be a slow and concentrated exercise. There is a lamentable sense of breathlessness about this essay. Perhaps a more deliberate, more expansive treatment of the 1860s would have avoided such startling observations as that Southern isolation removed the Negro "from the mainstream of Southern life of farm labor" (as if he were ever part of it). Certainly such an approach would have given us a greater sense of Professor Degler's established analytic and narrative gifts.

More satisfactory is W. David Lewis' appraisal of antebellum reform, which he depicts as a

concerted conservative effort to erect psychological substitutes—in the form of agrarian and revivalistic values—for the eroding aristocratic and religious values. His useful and fascinating essay is, I find, in the best tradition of historical scholarship. It investigates the powerful thrust of postrevolutionary conservatism, a phenomenon neglected by most historians, with the distinguished exceptions of James Morton Smith, Fred Somkin, and James Banner.

Nor does Clyde Griffen's contribution suffer from the superficiality sometimes attendant upon limitations of space. Providing a fresh dimension to the Progressive period, Professor Griffen is also aware that ideas are not created *ex nihilo* (in this case around 1900), but that their roots lie deep in earlier thought. He substantively connects the sentimentality of the 1840s with that of the Gilded Age, the old moral reformers with the new economic reformers, the antebellum climate of religious and moral evangelism (in which church and school stressed the old agrarian virtues of humility, self-reliance, frugality, and charity) with post war culture. But Professor Griffen also refuses to engage in the sort of retrospective symmetry John Roche warns us against; he notes how postbellum Protestantism accommodated to secular necessities and how, under the impact of Darwinism and industrialism, urban Protestant churches lost their intellectual rigor and doctrinal purity.

Loren Baritz, too, is at pains to suggest that history and culture, past and present, are part of the same seamless web. His essay, possibly the most balanced in the collection, resourcefully joins the interests and values of urban middle-class America with cultural radicals, but he also indicates how the two groups differed and how their attitudes and outlooks sharply contrasted with those of provincial America as well. Professor Baritz, for the most part, offers no surprises. The themes adumbrated here—rural dislocation, parochial opposition to change, prohibition as emblematic of God and country, the Klan's fear of alien values, the separate peace (in the form of withdrawal, alienation, expatriation) made by intellectuals and culture workers, the impact of Freud, the equation of primitivism and freedom, the discord between heart and head—are hardly innovating. But Baritz' graceful discussion of village and city,

his masterly integrative skill, his refusal to succumb to a vulgar reductionism—so easy in a short survey—make his essay outstanding.

The penultimate contribution (Cushing Strout's pastiche concludes the book), is by Warren Susman on the 1930s. It explores the autistic dreams of the subculture with splendid panache and inventiveness. It draws upon strikingly varied sources, including Erich Auerbach, John Strachey, George Orwell, Ruth Benedict, Karen Horney, Dale Carnegie, and Dashiell Hammett, as well as the decade's customarily invoked observers, such as Walter Lippmann and Randolph Bourne. Professor Susman's exploration of the innovative media of the thirties—the radio and the films—is unusual in itself; to relate them to established sociocultural myths, to suggest how they fed and reinforced fundamental conservative values and increased self-awareness of the national culture, is almost unprecedented among historians. One cannot say enough about the abundant merits of this essay as a provocative commentary on middle-class culture or as an examination of the manner in which myth is reinforced (by film, soap opera, and literature). Professor Susman displays a radical and highly unconventional imagination, an astonishing receptivity to the new, and he has enough here for a half-dozen forays into the 1930s. But all of the contributions are informed, readable, well organized, and suggestive of how far intellectual history has wandered from the old, well-marked path. They deserve a wide and appreciative audience.

MILTON CANTOR

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RICHARD HOFSTADTER and MICHAEL WALLACE, editors. *American Violence: A Documentary History*. New York: Alfred A. Knopf. 1970. Pp. xiv, 478, xiii. \$10.00.

WILLIAM M. TUTTLE, JR. *Race Riot: Chicago in the Red Summer of 1919*. (Studies in American Negro Life.) New York: Atheneum. 1970. Pp. xi, 305. \$8.95.

Near the end of his study Tuttle points out that all the social evils he sees precipitating the Chicago riot of 1919 remained unabated at its conclusion. Why then, he asks, did not rioting regularly recur in Chicago? He makes some suggestions about what "perhaps" happened, but

concludes that the solution probably lies in some "intangible factor, . . . something that might forever escape detection, . . . but something that might be worth more than all the studies and surveys of riots and violence put together." Such an oddly honest epitaph for this new field will hardly stem the approaching deluge of violence studies, but it harms no scholar to labor with some sense of his subject's mortality. Dry-as-dust unto Dry-as-dust. . . .

These two books offer two major approaches to the field of social violence and, both being competent and provocative, are suggestive of dilemmas endemic to the work. *Race Riot* is an able account of the events of one of the most destructive of post-World War I American riots and of its broad generating social causes: rapid black migration, ethnic hatreds, housing conflicts, labor problems, political corruption, and police racism. Its weakness is that Tuttle's causal structure moves creakingly from the constants of racial tension to the specifics of the riot because he gives so little attention to its immediate context. The problem shows up well in that area where Tuttle's research and conclusions are freshest, racial conflict in labor. Here he shows how union racism, black strike breaking, and capitalist manipulation of both promoted strife in Chicago, but labor's connection with the riot itself is not self-evident. Tuttle's own facts suggest that the summer of 1919 featured a rapid decrease in unemployment and substantial advances in the racial attitudes and black recruitment in the stockyard unions. Unless the riot were "caused" by a social factor that had remained largely constant since 1894 at least, one might argue that it owed more to fears aroused by the highly public attempt of certain Chicago unions to shuck racism than to job competition.

Despite assertions about writing history "from the bottom up," Tuttle makes no attempt to identify or classify those who rioted. Were they laborers? Were they from racially contested neighborhoods? Were they politically active? Did they belong to the Irish athletic clubs, or have connections with the real-estate interests that fomented other types of racial violence? Such evidence is hard to get but would have been more plentiful for the 1919 Chicago riot than most, and valuable inferences can be made from limited amounts of data. Tuttle

uses an interview to clarify the triggering event of the riot but makes no use of this approach regarding the causal problem. His failure to do more with the immediate context is especially lamentable because the general milieu of this riot has already been thoroughly explored in the best commission study of a riot ever done in the United States—*The Negro in Chicago* (1922). The photographs in *Race Riot*, all taken from the earlier study with “Negro” changed to “black” in the captions, are excellent, but reproduced so darkly that in instances it is hard to tell black from white; that’s too much integration.

The Hofstadter-Wallace book, a collection of well-chosen, brief primary accounts of incidents, gives a good cumulative sense of the extent and variety of the nation’s social violence, but the format allows little more than that. Incidents of destructiveness follow one another like scenes in a Jacobean play, always fascinating but repeating the tragedy rather than making it humanly comprehensible. Hence the main intellectual thrust of the book lies in Richard Hofstadter’s introduction.

Hofstadter’s central concern is clearly with the present, although he uses standard secondary sources to make some intelligent points about the past, especially the “conservatism” and ethnic quality of American violence. The burden of the essay is to show contemporary “sidewalk Sorels” that violence may produce good results but is more likely to be an excuse for tragic and mindless destruction or repression. To support violence without close heed to the “rational calculus of tactical probabilities,” Hofstadter eloquently argues, is to be personally self-indulgent rather than socially concerned. He also pleads for gun control, and against “participatory democracy” where vicious local forces could undercut the “advancing cosmopolitan sentiment” that has been marked by “the replacement of small time vigilantes by state authorities or national troops.” Hofstadter does not consider whether a large residue of individual and local power, maligned as it has been at times, may have helped prevent state terror of the Hitlerian or Stalinist sort.

The last reflections on American society of this major American historian are sobering. Hofstadter sees “the same basic pathology in American life” that led to the Civil War—un-

controlled growth, hatred of authority, poor leadership, and heterogeneity—as still flourishing. He concludes that the United States, never well governed, will probably “slouch onward into its uncertain future like some huge inarticulate beast, too much attainted by wounds and ailments to be robust, but too strong and resourceful to succumb.” If such a somber vision of the nation prevails among antirevolutionaries, the calculations of America’s “sidewalk Sorels” may be less fanciful than Hofstadter believed.

DAVID GRIMSTED

University of Maryland

MIRA WILKINS. *The Emergence of Multinational Enterprise: American Business Abroad from the Colonial Era to 1914*. Cambridge, Mass.: Harvard University Press. 1970. Pp. xiv, 310. \$9.50.

Giant, American-based corporations are in business throughout the world today, extracting raw materials, marshaling capital and labor, manufacturing and distributing goods, providing a variety of services. Recently, the activities of these firms have attracted considerable attention. Foreign nations have become more and more concerned about the political, economic, and even the social implications of the spread of powerful American companies. Domestic fears have been excited by talk of a military-industrial complex with vested interests in commercial expansionism. As yet, however, few historians have provided us with detailed information on the early development of these multinational enterprises, and even fewer have attempted to present a broad, synthetic view of the activities of American companies abroad.

With the publication of Mira Wilkins’ volume, we now have a carefully researched and well-written general account that surveys this subject from the colonial period through 1914. The author also promises a sequel that will cover the years since the beginning of World War I. In the present book she offers a substantial body of interesting and sometimes new data; but the main strength of the study is its synthesis of information heretofore scattered in secondary studies, government records, and business journals. She has also gone to company records and has interviewed businessmen in numerous firms. Wilkins finds evidence of significant patterns of direct investment long

before 1914, and indeed she concludes that the modern international firm emerged during the years 1865-92. Historians will also find instructive the emphasis that she places upon Canadian, as opposed to Caribbean, investment. Previous scholars seem to have unduly stressed the Caribbean because they were primarily interested in questions of foreign policy and not in economic problems, *per se*.

While the author's research is commendable, many readers will probably find her interpretations less worthy of praise. Throughout, the American corporation scores suspiciously high marks, whether the question involves cartelization in Europe or the exercise of politico-economic muscle in Latin America. Students of economic development will be disappointed by the author's tendency to dodge most of the tough analytical questions—for example, to determine the extent to which inordinately high profit rates on foreign investments tended to drain capital out of the countries involved and impede their long-run development. On controversial questions such as this, the author all too often merely points out that the evidence is thin or conflicting and leaves the reader to draw his own conclusions. Where Wilkins does offer evaluations, her sympathies clearly rest with the American companies, but I doubt that the historical indictment against United Fruit *et al.* will be quashed until readers are offered a more penetrating analysis than they receive in her study.

Disappointing as this may be, the book will still prove valuable to all those who are interested in the evolution of the modern firm and in the economic dimensions of American foreign policy. They will find here a well-documented volume on a subject of central concern.

LOUIS GALAMBOS
Johns Hopkins University

ANGIE DEBO. *A History of the Indians of the United States*. (The Civilization of the American Indian Series, Volume 106.) Norman: University of Oklahoma Press. 1970. Pp. xvii, 386. \$8.95.

A history of the Indians of the United States is an ambitious undertaking, one that very few persons have had the courage or the knowledge

to undertake. Anthropologists and popular writers interested mainly in Indian culture and civilization—Paul Radin, *The Story of the American Indian* (1934); Clark Wissler, *The Indians of the United States* (1940); Ruth Underhill, *Red Man's America* (1953); and Alvin Josephy, *The Indian Heritage of America* (1968)—have offered comprehensive studies of the Indians as people, but their works have been of slight value to persons who wanted to know what happened to Indians after the coming of the white man. The only broad histories of the Indians previously available have been Harold E. Fey and D'Arcy McNickle, *Indians and Other Americans* (1959), an interpretive account of Indian-white relations that is sympathetic to the Indians; and W. T. Hagan, *American Indians* (1961), a brief narrative history, objective in tone.

Angie Debo's history is a welcome addition to the sparse historical literature in this field. It surpasses both Hagan and Fey and McNickle in length and in detail. It is a balanced account, devoting approximately twenty per cent of its length to the colonial period, thirty per cent to the years 1776-1865, thirty per cent to the period 1865-1900, and twenty per cent to the twentieth century. It is particularly strong in its coverage of the Five Civilized Tribes and the reservation-allotment period. It contains a brilliant chapter on the natives of Alaska, bringing their history up to 1970, and it concludes with an astute evaluation of the Johnson-Nixon policies.

Despite its virtues Miss Debo's book is deficient in several respects. Her account of the reduction of the Plains Indians is confusing and difficult to follow. She passes lightly over all the southwestern tribes except the Apaches. And although this is true of nearly all studies of the Indians and not only of Miss Debo's, there is relatively little in her section on Indians in the twentieth century that goes beyond a surface description of events.

In her introduction Miss Debo acknowledges that she has placed "what may seem an undue emphasis on the history of Oklahoma Indians." She also says that her list of selected readings may have given too much attention to books published by the University of Oklahoma Press. I trust it is not unfair to say that I find myself

among those who will agree that the emphasis in both instances is misplaced. There are imbalances in the narrative occasioned by an emphasis upon the Five Civilized Tribes. There are major omissions in the bibliography that should not have been permitted, the most obvious of which are Edward E. Dale, *The Indians of the Southwest* (1949); W. T. Hagan, *American Indians*; Edward Spicer, *Cycles of Conquest* (1962); Loring B. Priest, *Uncle Sam's Stepchildren* (1942); and Robert M. Utley, *The Last Days of the Sioux Nation* (1963).

In summary I would say that Miss Debo's book is the best one-volume history of the Indians available today. It is an excellent place to begin the study of American Indians, and its reasonable price makes it all the more attractive. While it is not free from certain defects, those very defects should in themselves point the way to further histories and in some instances to further research.

LAWRENCE C. KELLY

North Texas State University

STANLEY F. CHYET. *Lopez of Newport: Colonial American Merchant Prince*. Detroit: Wayne State University Press. 1970. Pp. 246. \$8.95.

JACOB R. MARCUS. *The Colonial American Jew, 1492-1776*. In three volumes. Detroit: Wayne State University Press. 1970. Pp. xxxiv, 515; xi, 519-1110; ix, 1113-1650. \$45.00 the set.

According to Jacob R. Marcus only about five thousand individuals lived in colonial America who could be classified as Jews; most of them were obscure, and few of the others left papers that survive in substantial quantities. Stanley F. Chyet has used one of the richest collections in writing a biography of Aaron Lopez, with disappointing results, while Marcus has surmounted formidable obstacles to write a comprehensive study of Jews in the Western Hemisphere before the nineteenth century, putting heavy emphasis on North America. Parts of his work suffer from scarcity of evidence, parts from other defects, but a great deal is excellent.

Chyet's treatment of Lopez has some valuable sections. The best are on the Portuguese background of the main figure and his relatives, their resumption of Judaism, and their adjustment to eighteenth-century America. Even these

passages suffer from the author's excessive attention to circumcision and genealogy and his nagging dissatisfaction with the hero for failing to oppose slavery.

Evaluating Aaron Lopez as a specimen of Jewish manhood, however, was less important than explaining his career as a Newport merchant, a task that Chyet performed with scanty insight. He assembled little more than lists of what Lopez bought and sold, with whom he traded, and what local products he processed for export, added a clumsy narrative of how some of the pre-Revolutionary political events hampered Lopez, and dwelt on a few personal relationships that figured prominently in the business. The reader is left wondering about several basic questions such as how Lopez got started, how he acquired the reputation for probity that enabled him to draw large credits on British firms, or even whether he made a profit. The reader would scarcely guess that Lopez was the last, and probably the cleverest, in the series of colonials who failed in the attempt to establish regular direct commerce between Great Britain and Rhode Island, a fact that might have guided the biographer into more fruitful lines of inquiry and analysis. Bernard Bailyn and others have taught us how to pose useful questions about colonial commerce and its context in imperial affairs, but Chyet has not learned from them. Widely read in the Jewish history literature, he has not gone far in his study of colonial history beyond volumes in the *Chronicles of America* series and old compilations of documents for classroom use.

Jacob R. Marcus' work has sections that reveal a similar obliviousness to modern scholarship on the colonial period and the guidance it could give for framing good questions of his data. This deficiency appears most often in his long discussion of "The Economic Activity of the Jew in British North America," which contains many enumerations of commodities, occupations, and the like, using rather sterile taxonomic categories. A thorough understanding of the structure of colonial trade might have inspired a better plan. All the same, he wrote fine passages in this section and he had much more to say about Aaron Lopez than Chyet did. Fortunately, Marcus often made

up for shortages in his stock of general knowledge by the critical intelligence and wisdom he brought to his research on Jews. And where it counted most, in the analysis of religious life, he was sufficiently well versed in Gentile ways to make perceptive comparisons.

The organization of Marcus' book requires further comment. After several captivating chapters on Brazilian and Caribbean Jewish communities, it treats North America in five main subdivisions: settlement, legal and political rights, economic life, Jewish communal life, and "The Jew in the Larger Community." Each of these is dissected meticulously into subtopics and sub-subtopics, thus insuring comprehensive coverage and a form of clarity. Completeness sometimes is mere base-touching, however. Systematically traversing the roster of colonies in one topic after another hardly seems justified, because several colonies contained too few Jews to be worth mentioning. Likewise, clarity can be pseudoclarity, as when a series of types of commerce is presented without consideration of the fact that in any man's business the different types were ordinarily interconnected.

The catalog effect created by the elaborate framework should not obscure the intriguing duality of Marcus' conception of his subject. At first glance the work looks like another in the old tradition of lengthy tomes on the few colonial forerunners of an immigrant horde arriving in later times, books designed to defend the later comers by showing that the stock had been on the scene at least as long as the WASPs and had done more than its proportional share in building the nation. Authors of these treatises have, traditionally, balanced a lack of humor and perspective with a surplus of zeal for stretching evidence. Not so with Marcus: though he used an old genre, he avoided the old failings, while remaining sympathetic to his subject. He necessarily deliberated on claims that Columbus was a Jew, but with strict objectivity; similarly, the very sobriety of his evaluation of Jews in the Revolution made his concluding section on that subject an anticlimax. Actually, in the old genre he employed concepts promulgated by modern scholars. Except for including a discussion of legal and political rights, Marcus framed his subdivisions in rough parallel to Handlin's categories of immigrant experience.

The exception is significant, because Marcus showed the colonial Jews forming not an ethnic group but a "people," using a term and concept also employed by their Quaker neighbors. A "people" might be defined by religion, ethnic ties, or both, but it constituted a semi-autonomous social group and had among its functions the task of obtaining and enlarging whatever legal privileges its members needed to retain their affiliation while participating in society at large. Moreover, Jews in the colonies had diverse ethnic backgrounds, even within the two religious traditions, the Ashkenazic and Sephardic. The prevalence of the Sephardic rituals after the Iberian elements had been outnumbered, Marcus argues, has obscured the fact that people of the different origins joined on equal terms in creating a distinctive American form of Jewish life.

For all the unevenness in the book, the thorough research and mature scholarship of *The Colonial American Jew* make it an important work. It will be mined with high yield by specialists on colonial and religious history. It provides a great improvement over some earlier studies that have been standard for want of anything better. If Marcus has not written the last word on early American Jewry—a scholar of his caliber would scarcely imagine that anyone could—he has lifted study of the subject to a plane of excellence where further work can be done that will be a tribute to his accomplishment.

SYDNEY V. JAMES
University of Iowa

CHARLES E. CLARK. *The Eastern Frontier: The Settlement of Northern New England, 1610-1763*. New York: Alfred A. Knopf. 1970. Pp. xxiv, 419, xxiv. \$10.00.

This exceedingly readable account of the settlements of southern New Hampshire and southern Maine before 1763 emphasizes the development of a distinctive culture derived from the interaction of the varied groups of people who came there. They were mobile and in part transient, with an environment dominated by sea and forest and by their products, fish and lumber—all accessible by water to Boston, the center of New England's population and power. It is a much needed synthesis of a subject so extensively documented and so com-

plicated in the interrelationships of the governments and people as to defy all but the most diligent and bold of investigators. Its provocative generalizations should provide topics for detailed studies that have been signally lacking in this geographic area. Large masses of court records, deeds, and biographical data remain to be exploited. Comparative studies with other sections of New England may further test the alternative view that, in spite of much colorful diversity within New England, Yankees share a common inheritance that the differences only make more interesting. Maine, east of Casco Bay, Essex County, Cape Cod, as well as other sections, will always claim, along with Clark's New Hampshire and York County, Maine, their separate identity.

Including as it does all phases of what we call "civilization"—religion, education, politics, careers and crafts, architecture—the book is a masterly description and analysis of the settlement and growth of a wilderness into what Hakluyt would have called "civilitie."

The author's use of the terms "imperialism" and "colonial policy" when referring to Massachusetts' relations with Maine I found confusing rather than clarifying. I missed, too, a few participants in the drama that Professor Clark may have intentionally omitted: Captain Francis Champernowne of ancient lineage; the Rev. Hugh Peter, the Puritan priest and later regicide, who played a considerable part in bringing New Hampshire into the Puritan fold; "Handkerchief" Moody, the veiled parson of Hawthorne's *Twice-Told Tales*; Charles Frost of Kittery, whose estate was appraised at £10,000 in 1725; Esther Wheelwright, the captive girl who became the mother superior of the Ursuline Convent in Quebec. And it is unfortunate that the author depended so completely in the early chapters of his book on the secondary works and documentary series long in print, admirable though they are. The use of more recent studies might have led him to a less provincial view of Sir Ferdinando Gorges' colonial projects and to credit the proprietor's deputy governor, Thomas Gorges, with laying a sound basis for the degree of freedom of religion provided by Maine's act of 1649. Nine years before this he told John Wheelwright, who was considering moving his settlers from Exeter to Wells, that "we forced

noe man to the common prayer booke or to the ceremonies of the Church of England but allowed the Libertie of Conscience in this particular." Nor would he have used Sullivan's statement that there were fifty families at Sheepscot Farms in 1630.

The illustrations are well chosen, particularly the maps of the land divisions in the towns. Unfortunately, the index does not adequately represent the richness of the book's contents.

ROBERT E. MOODY
Boston University

RAPHAEL N. HAMILTON, S.J. *Marquette's Explorations: The Narratives Reexamined*. Madison: University of Wisconsin Press. 1970. Pp. xv, 275. \$10.00.

On his return to Montreal in 1674 after his voyage to the Mississippi River, Louis Jolliet lost his journals and maps when his canoe overturned within sight of Montreal. A year earlier church-state conflict in France brought an end to the orderly publication of the *Jesuit Relations* of New France through which Father Marquette's account of the same voyage might normally have been reported. Nevertheless, in 1681 Melchisédech Thévenot included in his *Recueil des voyages* an account of the Marquette-Jolliet expedition without indicating the source of his narrative. From that beginning the literature about the French discovery of the Mississippi has grown, and scholars have steadily uncovered new sources, edited them, altered them, and doubted them. There was sufficient question about the authenticity of some of the primary documents to bring Father Francis B. Steck to the conclusion, expressed in two books, that Marquette's part in the expedition had been magnified and that sources attributed to him were not in his hand at all.

Now Father Raphael N. Hamilton has gone back to the fundamental work of the historian—the authentication of the documents on which the Marquette story is based. One need not know or care about the exploration of mid-America to appreciate this book. It is an example of historical method applicable to any number of problems in historical research. Indeed, it invites us to take another look at problems we have considered solved. Starting the historiography of Marquette's voyage with

Thévenot, Father Hamilton leads us through the works of some distinguished historians: Father Charlevoix, Benjamin Franklin French, Father Felix Martin, Edmund J. O'Callaghan, John Gilmary Shea, Father Fortuné M. de Montézon, Reuben G. Thwaites, Jean Delanglez, Pierre Margry, and Francis Parkman, among others. Yet none of these historians applied the rigorous tests of authenticating handwriting, paper, or historical context to the basic documents as Father Hamilton has done. The chief issue is a manuscript titled *Recit des voyages et des découvertes du P. Jaques Marquette*, which was left in the hands of some Montreal nuns by a dying priest in the eighteenth century, and another of a similar title that has migrated through half a dozen European archives.

In examining these and related documents Father Hamilton leaves nothing undone, exposes some less than admirable work by some of his predecessors, informs us richly of Jesuit history and publishing policies, and removes all doubts about the validity of the sources from which the story of the Marquette-Jolliet expedition has been told.

JOHN PARKER
University of Minnesota,
Minneapolis

JOSEPH and NESTA EWAN. *John Banister and His Natural History of Virginia, 1678-1692*. Urbana: University of Illinois Press. 1970. Pp. xxx, 485. \$15.00.

John Banister (1650-92) is best remembered as a late seventeenth-century Virginia parson who contributed some American plants to John Ray's *Historia Plantarum* (1680) and a few articles to the *Philosophical Transactions* (1693-94) before he met an untimely death, presumably from a fall. The Ewans show not only that the cause of his death was reported incorrectly, as is the case with most of the details of his life, but also that Banister was important for what others borrowed from him. Most significantly for historians, they demonstrate that he was the unacknowledged author of most of the natural history and ethnological sections of Robert Beverley's *History and Present State of Virginia* (1705).

This is not a biography, although the first section places Banister in the context of his English contemporaries in natural history and

gives what can be ascertained about the life of this Oxford don sent to Virginia by Henry Compton, bishop of London and botanist, apparently for the collection of rarities as much as for the cure of souls. The remainder of the book is devoted to a carefully edited compilation of Banister's unpublished writings, to the known contents of his library, and to the other sources that he used.

The result of all this is to show that Banister was a far better naturalist than any working in the mainland English colonies in the seventeenth century, or, for that matter, through most of the colonial period. This might have been an exercise in historical futility had not so many of his materials been used by others: Robert Morison and John Ray, among the major figures, but also by such minor ones as James Petiver, Leonard Plukenet, and William Sherard. Through them, sometimes at second or third hand, Banister's materials entered scientific literature. This is particularly evident in the Ewans' editing of Banister's "Plant Catalogue," where they have found their way with great skill through the thickets of pre-Linnaean nomenclature.

The major weakness of the book is that it is made up of many parts and does not really constitute a whole. This is evident in the typographical variety between the introductory materials and the edited section and is present, also in a mechanical sense, in the several kinds of annotations used. It is, of course, largely due to the nature of the problem; but the Ewans have contributed also in their cautious failure to go much beyond the bare results of their research into interpretation. They have, nonetheless, accomplished a major task of historical restoration.

GEORGE F. FRICK
University of Delaware

JOHN G. CLARK. *New Orleans, 1718-1812: An Economic History*. Baton Rouge: Louisiana State University Press. 1970. Pp. xii, 395. \$10.00.

By 1812 New Orleans was the fifth largest American city, possessed a thriving export-import trade, and boasted a vast commercial hinterland. The Crescent City's first century, however, had been characterized by slow economic progress for long periods, with occasional advances set back by wars, floods, hurricanes, epidemics, scarcity of money and immigrants,

and the region's low priority in the colonial system of France and later Spain. During the late Spanish era the influx of Americans brought the city the human and monetary resources and entrepreneurial talents it needed for sustained growth. The huge hinterland of the upper Mississippi Valley was growing rapidly in population and agricultural productivity by the 1790s, while in the lower valley cotton and sugar were becoming important export staples, all of which abetted New Orleans' rise as a port and commercial center. Despite continuing money and credit shortages and the trade problems accompanying Anglo-American friction during the years 1803-12, the city at last possessed the dynamic for the economic expansion it would experience in later decades.

Clark's work, the first scholarly monograph on New Orleans' economy from the founding of the city to the War of 1812, is based on a thorough mastery of the published and manuscript sources. Although his style is sometimes pedestrian, his analyses are lucid and often perceptive. Clark shows unusual ability to comprehend and explain the larger picture of forces and factors outside New Orleans itself that were significant in shaping the city's economy. But so much detail is given on some aspects of the hinterlands, West Indies, French trading houses, and international rivalries in the Gulf region that the reader may wonder whether Clark will ever concentrate on commercial life within the city. Perhaps much of his material on the French and early Spanish eras, when the city's economic interests were not distinctive from those of the Louisiana colony, could have been condensed. In dealing with the 1790s and the American territorial period Clark focuses more sharply on the economic activities and business leadership of the city itself.

The book is remarkably free of typographical gremlins. The dozen tables of economic data are helpful, but a map should also have been added. Most researchers will lament the almost total absence of footnotes that cite exact sources. Clark's bibliographical notes are valuable, but orthodox footnoting would have done more justice to his exhaustive research and better assisted the researcher using his book.

D. CLAYTON JAMES
Mississippi State University

JERE R. DANIELL. *Experiment in Republicanism: New Hampshire Politics and the American Revolution, 1741-1794*. Cambridge, Mass.: Harvard University Press. 1970. Pp. xiv, 261. \$10.00.

Professor Jere Daniell of Dartmouth College has written a workmanlike volume about the revolutionary era of the state in which he teaches. His multicausal explanation of the forces affecting the political culture of New Hampshire before and after the Revolution includes both general and specific causes of events. In his description of general causes—the role that ideology, constitutional beliefs, irrational fears, kinship patterns, and localism played in shaping political attitudes and behavior—he relies heavily upon an intellectual history approach and reveals the influence of his mentor, Bernard Bailyn. When Daniell turns to specific causes resulting from conditions unique to New Hampshire, however, he is prone to resort to a more materialistic approach—the economic split between the interests of the revolutionary leaders and their colonial counterparts; the geographical division between coastal and interior regions as well as river valleys as opposed to other areas; and the population explosion that saw the state's growth spiral from 40,000 at mid-century to 140,000 by 1790. Hence, he concludes, there is merit in both the older interpretation of the Progressive historians and that of their recent critics as exemplified by his mentor.

There are advantages and disadvantages in the eclectic stand he has adopted. By searching for both conflict and consensus, the continuation of the old as well as emergence of new social groupings, and the particular on the local and state level rather than the general on the national, he has added to our store of knowledge. But by not facing squarely the philosophical question of whether men's political actions are primarily motivated by economic self-interest or ideological considerations, he has undermined the possible significance of his findings.

GEORGE A. BILLIAS
Clark University

HELEN R. PINKNEY. *Christopher Gore: Federalist of Massachusetts, 1758-1827*. Waltham, Mass.: Gore Place Society; distrib. by Barre Publishing Company, Barre, Mass. 1969. Pp. 180. Cloth \$10.00, paper \$3.95.

This is the first biography of the almost forgotten Federalist Christopher Gore, graduate of Harvard in 1776, successful lawyer, speculator, banker, diplomat, and a member of the Unitarian King's Chapel. His public service included such noted offices as commissioner under the Jay Treaty, the governorship of Massachusetts, and senator of the United States. This is then the story of a second-rank Federalist whose life style was congruent with his beliefs: he enjoyed serving "the class which is able to pay," favored the postrevolutionary deflation as "many who retail tape & pins must, as they ought to have done years ago, return to labour," and believed in pomp, property, privilege, and order. Gore's idea of a political campaign was to ride through the state in an open carriage, surrounded by servants in livery; as expected, this son of a Tory fawned over aristocrats, hated the French Revolution, praised the restoration of a Bourbon "to the throne of his ancestors," and told Harvard protestors to submit to authority. If Gore's style reveals none of the ambiguity of an Alexander Hamilton nor the subtlety of a John Adams, it does tell a great deal about acceptable Federalist behavior. Our task is not to caricature Gore because his political sentiments sound alien to our ears but rather to fathom his world and that of the Massachusetts voters who supported and reified it. Nor are we interested in showing that the Federalists were right and the opposition wrong. Dr. Helen R. Pinkney in this well-written book, originally a 1944 Radcliffe thesis, has collected an enormous amount of information. And by and large she has presented Gore as he wanted to be seen. Yet this traditional approach, as well as the absence of any visible dependence upon more recent research on personality types and party politics, gives a very dated cast to this study.

JOHN J. WATERS

Charles Warren Center

CHARLES COLEMAN SELLERS. *Charles Willson Peale*. New York: Charles Scribner's Sons. 1969. Pp. xiv, 510. \$20.00.

From the late 1760s until around 1795, Charles Willson Peale was one of the foremost portrait painters of colonial Americans, their Revolutionary War heroes, and the men and women

who created a new nation in the years following the war. Between Copley's permanent departure in 1775 and the return of Gilbert Stuart in 1793, Peale was the unrivaled master of American portraiture. Those who now wish to see the likeness of the leading citizens of those decades portrayed with the forthrightness and the self-confidence that echo perfectly the philosophy of Benjamin Franklin turn first to the portraits by Charles Willson Peale. Peale, who was continually at the center of things, knew nearly every great personage of his day, and he painted honest, unidealized likenesses of Washington, Jefferson, Franklin, and hundreds more. The proud merchants and craftsmen of late eighteenth-century America who had their appearances immortalized by the jovial and industrious Peale, do indeed present a visualization of the practical philosophy of Franklin that if one works hard, is frugal, and enjoys the blessings of the Almighty, one will have an enjoyable life on this earth. These men and their ladies were proud of their earthly accomplishments and wanted them indicated in their portraits, and Peale complied by including a finely crafted Chippendale table, an exquisite silver ink stand, or some beautiful specimen of fabric in the attire. How very different the character of these men and women appears from the dark, somber, often gloomy portraits of the seventeenth century.

But Peale's career did not end in 1795, of course; it only changed its direction. From at least as early as 1785 Peale had been interested in a museum. In 1795 he turned his practice of portraiture over to his young son, Rembrandt Peale (a personality almost as fascinating as his father), and Charles Willson devoted himself to his famous museum in Philadelphia. Thereafter, he painted only things that interested him such as *The Exhumation of the Mastodon* (1806) and *The Artist in His Museum* (1822), both of which are among the many fine color plates in Sellers' book. In the latter painting, Peale lifts a great curtain and beckons the viewer to enter and see the extraordinary art and natural history museum he has created: the great skeleton of the mastodon is there, a stuffed wild turkey, and row upon row of other stuffed creatures in boxes with natural settings, while at the top of the wall is his collection of portraits of famous men. Here before us is Charles Willson Peale himself and a collection

of things that was formed by one of the most brilliant men of a spectacular era.

Those who are interested in learning more of the public and private life, of the inventive mind, and of the artistic achievement of this unusual man can find no better authority than Charles Coleman Sellers, librarian at Dickinson College and a descendant of his subject. Some will already be familiar with his two-volume work *Charles Willson Peale* (1947) and his *Portraits and Miniatures by Charles Willson Peale* (1952), both published by the American Philosophical Society. Now these are followed by this handsome, well-written, and well-illustrated volume that brings together from the author's vast knowledge of his subject and from an enormous body of documentary material (letters, journals, articles, and so forth) a splendid study of the man, his life, his work, his family, and his friends. It is, as the title states, a biography; the reader is led through the artist's youthful years, the trials of his early days as a painter, his patriotic zeal during the Revolutionary War, and his happy days of portrait painting and museum making in Philadelphia to a moving concluding chapter. There follows a genealogy of the Peale family and notes and bibliography. There are 102 half-tone illustrations and thirty color plates. One can only wish more early American artists were known to us through such thorough, erudite, and sympathetic studies as this.

WAYNE CRAVEN
University of Delaware

RICHARD D. BROWN, *Revolutionary Politics in Massachusetts: The Boston Committee of Correspondence and the Towns, 1772-1774*. Cambridge, Mass.: Harvard University Press. 1970. Pp. xiv, 282. \$10.00.

Until quite recently historians viewed the Massachusetts committees of correspondence as revolutionary cells, cranking out radical propaganda and plotting violent deeds under the surreptitious domination of Samuel Adams and his Boston committee. Now in a sharply focused study Richard D. Brown of Oberlin College has given us a more balanced judgment of the Boston committee's relationship with the other towns in the coming of the Revolution. "The [Boston] committee of correspondence does not appear to have converted Massachusetts to its

own views," Brown concludes, "but rather to have stimulated townspeople to express their own similar beliefs" (p. 245). Brown has made full use of the Boston committee's records and has also examined the records of nearly 100 towns, the papers of major figures of the period, and a wide range of printed sources, including broadsides, election sermons, and newspapers. Because the work is analytical rather than narrative, the style is somewhat stiff, but Brown has given us a most useful study nonetheless.

The first committee of correspondence was created by the Boston town meeting on a motion by Sam Adams in November 1772 "to state the Rights of the Colonists and of this Province in particular" and to communicate with other towns. Its first report, entitled *The Votes and Proceedings of the Town of Boston*, claimed that government officials were in effect servants of the people. The Bostonians therefore took particular exception to Parliament's raising a revenue in the colony by taxation for the purpose of paying the salaries of the governor and other royal officials. Publication of the pamphlet provoked Governor Hutchinson into an intemperate reply before the General Court in January 1773; more important, it stimulated local towns to ponder the issues raised by both Boston and the governor. By the end of summer 1773, over half of the colony's 260 towns had taken some sort of action, although only 58 had actually established their own committees of correspondence.

High tide for the Boston committee came with the crisis over tea in late 1773, although Brown is reluctant to assign it more than "some share of the responsibility for the Tea Party" (p. 164). When news of the Boston Port Act arrived in May 1774, the committee proposed a uniform boycott of British goods under a "Solemn League and Covenant," but this time the Bostonians overstepped the fine line between guiding and goading the other towns into action. The idea fell through. The Boston committee had done its work well, however. By the fall of 1774, when county conventions and the provincial congress took over leadership of the resistance movement, political activism had replaced apathy on the local level, and a new revolutionary consensus had been reached in Massachusetts.

BENJAMIN W. LABAREE
Williams College

DAVID SYRETT. *Shipping and the American War, 1775-83: A Study of British Transport Organization*. (University of London Historical Studies, Number 27.) [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1970. Pp. x, 274. \$10.60.

Shipping and the American War, in the author's words, "constitutes an attempt to illuminate the workings of the Navy Board's transport service which was a small, but essential, component of the British war machine." The transport service "was of great strategic and logistical importance" and its history involves not simply "a tale of military operations and logistics, but rather the story of the interaction of strategy, logistics, administration, and economics." Syrett centers his study on the activities of the Navy Board rather than on Whitehall or the military commanders in the field because "it fell to the Commissioners of the Navy to adjust those conflicting forces [strategic desires, military necessity, economic reality, and administrative possibility] into a coherent plan of action for the transport service."

The end result is a technical study that is well written and exhaustively researched but that leaves one wondering whether it might not have been better had Syrett, with less expenditure of his and the reader's time, settled for an article rather than a book. Had he concentrated more, as he promised in his preface, on "the interaction of strategy, logistics, administration and economics" my reaction might have been different. But, unfortunately, I gained very little in the way of rewarding new insights into the general problems of the English government in their conduct of the American War. Instead I suspect the reader will learn, as I did, rather more than he might care to about questions concerning tonnage and the measurement and inspection of shipping and the physical transport of troops and supplies.

The concluding sentence of *Shipping and the American War* suggests that by any standard "the achievements of the transport service during the American War rank with the greatest military and administrative feats of the eighteenth century." But earlier it was indicated that only the "end of the war in the beginning of 1783 prevented a catastrophe." Exactly how the two statements are to be reconciled is left

to the reader. The truism concerning over-absorption in separate "trees" leading to an inability to see the "forest" as a whole is particularly relevant to technical history of the kind Syrett offers here. There is no arguing with his facts or his research. But why those facts should be of importance to anyone is something else again.

Early in *Shipping and the American War* (on page 1, to be exact), Syrett suggests that prior to the war the British system, which "may have been an art but was never a science," had worked, "for until the outbreak of the American War it helped Britain to become—or at least it had not prevented Britain from becoming—one of the great powers of the world." What Syrett misses is that it was, on the contrary, the lack of system that had facilitated British advances prior to 1775; and that, consequently, England's fundamental wartime problem, in the American War as well as earlier conflicts, was to adapt its basically private commercial economic structures to the needs of collective national military effort. In other words, the English government had to create a system, administrative and otherwise, where before there had been none, or very little. This was not accomplished in time to influence the outcome of the American War. But the lessons learned were not lost, and they had a great deal to do with England's success, a generation later, in the contest with Napoleon's France.

To be fair very few technical histories of the kind Syrett offers here are as well written and as carefully researched as this one. It is with the kind of history, rather than this specific study, that I would argue. What he set out to do, Syrett has done very well. For the rest, my reaction is personal and, I am sure, as questionable as any of the problems I have raised in the process of coming to terms with Syrett's *Shipping and the American War, 1775-83*.

THOMAS C. BARROW
Clark University

RONALD F. BANKS. *Maine Becomes a State: The Movement to Separate Maine from Massachusetts, 1785-1820*. Middletown, Conn.: Wesleyan University Press for the Maine Historical Society. 1970. Pp. xx, 425. \$15.00.

During the eighteenth century Sir Ferdinando Gorges' province of Maine and the adjacent

eastern lands claimed by both England and France came under the government of the land-hungry, power-conscious Puritans of Massachusetts Bay, notwithstanding the counterclaims of private proprietors and the orders of Charles II himself. In 1820 the people of the district, overriding the ties of kinship and economic interest that had bound them so long to Massachusetts, successfully followed prescribed constitutional procedures—the vote of the people of Maine, the consent of the legislature of Massachusetts, the approval of the national Congress—and became a separate state. Now, one hundred and fifty years later, the Maine Historical Society appropriately celebrates the event by sponsoring this first full account of the forty years of arguments, political maneuverings, and votes that brought the new state into being. With its full appendixes and analyses of votes the volume at once takes its place as the standard account, unlikely soon to be superseded.

In most ways the act of separation was an anticlimax. From its earliest days Maine was a separate geographic concept, its separateness accentuated by the stretch of New Hampshire seacoast that lay between the settlements of Maine and Massachusetts. The many attempts made by outside interests to set up jurisdictions there kept the possibility of separation in view. The successful example of Vermont was suggestive. The issues had become old by 1820, most of the early grievances having been remedied by time or legislation. The "Ohio fever" had diverted attention from the long-hoped-for Maine land boom. And in the end the intrusion of Missouri and the slavery question had a tarnishing effect upon the achievement.

The arguments for statehood—geography, population, the need for a more immediately responsive government and court system, Boston's economic domination, religious equality, land and tax policies—were freely discussed by the press in Maine and Massachusetts. Unfortunately, studies in depth of these subjects have not been made. Hence it is impossible to develop a clear thesis of the relationship between Jeffersonian theories and the achievement of statehood that is suggested by the pattern of voting. Professor Banks finds some of the Democratic party's ideas of democratic government in the new state constitution, which he

analyzes in detail. Even better evidence might come from a study of Maine in the first few years after separation. It is pleasing to know that the new state chose Charles Bulfinch to design its statehouse and that the supervisors built it for less than the amount appropriated.

Most obviously the launching of the new state was the result of sustained efforts by the local politicians, for the most part Democratic, led by William King, who alone seems to have a claim to be regarded as a statesman. Their reward was elective office for themselves and patronage for their friends. Yet the political game was complicated by the crosscurrents of Democratic and Federalist interests in both parts of the commonwealth; neither party in Massachusetts proper wished to give up the support of its members in Maine. By 1820 both were seeking new directions.

ROBERT E. MOODY
Boston University

ROBERT GRIFFITH. *The Politics of Fear: Joseph R. McCarthy and the Senate*. Lexington: University Press of Kentucky for the Organization of American Historians. 1970. Pp. xi, 362. \$8.50.

SEYMOUR MARTIN LIPSET and EARL RAAB. *The Politics of Unreason: Right-Wing Extremism in America, 1790-1970*. (Patterns of American Prejudice Series, Volume 5.) New York: Harper and Row. 1970. Pp. xxiv, 547. \$12.50.

NUMAN V. BARTLEY. *From Thurmond to Wallace: Political Tendencies in Georgia, 1948-1968*. Baltimore: Johns Hopkins Press. 1970. Pp. viii, 117. \$6.95.

Robert Griffith's book is a highly valuable addition to our knowledge of power politics and the "anti-communist impulse" in the 1950s and is a deserving winner of the Frederick Jackson Turner Award for 1970. It is a carefully detailed and well-written examination of the rise and fall of Joseph McCarthy, emphasizing his interaction with the traditions and prejudices of the United States Senate.

Originally a doctoral dissertation at the University of Wisconsin, the book is grounded upon exhaustive research. The author studied every relevant manuscript collection now open to scholars (the files of the National Committee for an Effective Congress were especially rich), combed a wide range of congressional reports, newspapers, and magazines for facts and in-

sights, and read almost all of the useful secondary literature on the areas of his concern. His bibliographic essay is the best of its kind. One might only complain about the curious lack of oral interviews; numerous senators who supported and fought McCarthy are still alive, and I have discovered several who are willing to share their memories with historians. (Moreover, someone should have caught the use of "confidential source" to document serious charges against McCarthy and Joseph Welch. That is the sort of evidence McCarthy himself frequently relied upon.)

Griffith's thesis is sound: McCarthyism was a crude political tactic employed by Republicans to gather votes from Americans, fearful of the cold war and long suspicious of radicals, with charges of internal Communist subversion. Joe McCarthy was a talented and unprincipled slugger who stumbled upon the tactic after it had proven effective and was tolerated and encouraged by Republicans as long as he was thought useful. A timid and bumbling Senate looked on through the almost four years of McCarthy's rampage: Democrats afraid to be labeled "soft" on communism, Republicans unwilling to expose their major political issue as a fraud. "Caution became contagious, and the politic actions of fearful men ensured McCarthy's continued power and influence." Eisenhower "was a decent man, but he possessed neither the ability nor the inclination to deal with a political problem of this complexity." The Senate's flaccid vote of condemnation in late 1954 occurred only after McCarthy's national popularity had been shattered, and even then only after herculean efforts by organized establishment liberals to stir the likes of Lyndon Johnson.

In the course of the study the critical issue of the China Lobby's contribution to the hysteria of these years is resurrected. Paul G. Hoffman's vital behind-the-scenes role in the fall of McCarthy is discovered. The mass media's contribution to McCarthy's career is judiciously weighed. And the author reveals a refreshingly sophisticated knowledge of the Senate's creaking and often ineffective machinery.

Of course McCarthyism was more than a vote-getting device; it was also a tool of "true believers" and hucksters who used it in many walks of life to blackmail, coerce, censor, and

destroy. Griffith's book might have come closer to the full study of McCarthyism we still need had it inquired more deeply into the full range of the fear that struck senators dumb.

Progress along this line is made in *The Politics of Unreason*, an effort by two veteran sociologists, with the support of the Anti-Defamation League of B'nai B'rith, to produce a comprehensive analysis of right-wing extremism in American history. It is a bold, provocative, very important book, more often than not unpersuasive, guaranteed to stir controversy.

"The genius of the American society," the authors exclaim, "is that it has legitimized ambiguity." It follows then that "extremism basically describes that impulse which is inimical to a pluralism of interests and groups, inimical to a system of many nonsubmissive centers of power and areas of privacy. . . . The operational heart of extremism is the repression of difference and dissent, the closing down of the market place of ideas." "Right-wing" is equated with "preservatist or restorative tendencies which somehow limit new access to power and status."

The familiar concept of status displacement, the authors charge, is the key to right-wing extremism in America. Under examination are (among others) the Anti-Masons, the Know-Nothings, the American Protective Association, the Ku Klux Klan, McCarthyism, the John Birch Society, and the George Wallace boom. Common to each movement were status strains at higher levels of society, social discontent at lower levels (there must be an elite leadership and a mass base for an effective right-wing movement), a common corporeal target (the object of a conspiracy theory), and a background of shifting political loyalties. Protestant nativism was at the core of most preservatist discontent prior to the 1930s, and thereafter it was mostly nationalism and an abstract sort of racism—"un-Protestant" was replaced by "un-American."

Almost all of the movements scrutinized fit the broad formula (the American Protective Association is the most tentative), or at least seem to in lieu of further research. In preparing this study Lipset and Raab consulted a massive quantity of historical and sociological sources and employed a large number of unpublished doctoral dissertations. Historians will

note that the book is too long and littered with jargon, but greater sins have been charged to sociologists.

Most stimulating, although I think basically wrong, is the contention that McCarthyism was not really a right-wing extremist movement. It was a "tendency of the times," "more a hysteria than a political movement." McCarthy and his supporters failed to lash out against specific, identifiable groups of traitors. The senator's approach to politics was "apolitical." Moreover, there was substance behind McCarthyite charges: "there *were* traitors, there *were* spies, there *were* some significantly placed Communist cells in America."

Another study that examines the effects of status on political behavior is Numan Bartley's statistical analysis of voting tendencies in Georgia since 1948. Some forecasters had predicted that with the demise of the one-party system in the South the Populist vision of an alliance of have-nots would become a reality: an interracial union solidified behind a program of economic and social reform. But, as Bartley shows, this is not taking place in Georgia. The GOP has increasingly become in the 1960s the party of affluent middle-class whites in the cities and suburbs, while George Wallace has claimed the allegiance of white rural voters. The Democrats have benefited from increased Negro voter registration and still win the support of white liberals, but their strength at the polls, at least in presidential elections, is waning badly. Harry Truman won sixty-one per cent of Georgia's votes in 1948, while Hubert Humphrey could claim a mere twenty-seven per cent in 1968.

Republicans and Wallaceites share a desire for laissez-faire government and white supremacy, and their merger is not at all unlikely: "Assuming the absence of a viable Wallace-type third party alternative, it seems probable that future Republican presidential candidates can expect substantial support from rural-small-town-lower status white voters."

One might hope that rural whites would be eager to accept federal efforts to help them (and their black neighbors) break the grinding poverty that has so long plagued their existence. But, as Bartley explains it, "their declining economic and social status has made them more than ever the great conservators of the South's

traditions, and they have lost much of the economic radicalism that once made them the cutting edge of southern reform."

This is an able monograph which scholars will find useful as a footnote to V. O. Key, Jr.'s *Southern Politics*.

THOMAS C. REEVES
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Parkside*

GEORGE C. ROGERS, JR. *The History of Georgetown County, South Carolina*. Columbia: University of South Carolina Press. 1970. Pp. xvi, 565. \$12.50.

The history of Georgetown County, South Carolina, is the history of the rise, flourishing, and decline of two of the South's great staple crops—indigo and rice. Winyah Bay and its tributaries gave the area a geographical unity that made it possible for a distinct aristocratic society to develop over a period of 130 years. Between 1850 and 1860 the Georgetown rice planters, a small, closed group knit together by marriages and common interests, reached the peak of their wealth, power, and influence. They dominated the district and to some extent the state as well. "It was as though the entire Georgetown story led up to this summit," writes George C. Rogers, Jr., "the brief halcyon period before the fall."

During the nullification crisis Georgetown, with its population eighty-nine per cent slave, was ready to be organized into a militant statewide movement. Rogers says that "William W. Freehling is correct that fears concerning the future of slavery were etched deeply into the planters' mind." But Rogers fails to extend this insight to the later secession movement, suggesting instead that South Carolina seceded because of its self-reliant spirit and romanticized revolutionary past.

The post-Civil War years in Georgetown saw the collapse of the rice economy by 1911 and the even more rapid disappearance of the rice planters as a power elite. A new economic leadership, "drawn from the middle ranks of the pre-war society or from the ranks of entirely new men," emerged in the 1880s based on lumbering, rice milling, transportation, and distribution. Beginning in the 1890s rich Yankees moved in to buy the rice plantations, "all with historic pasts and appropriate settings for their

gentlemanly sports." This second Yankee invasion ended with World War II; after 1945 the estates became subdivisions, pine lands, cattle and egg farms, and tourist attractions. Since 1936 the International Paper Company has been the dominant economic force in the county.

Rogers, a student of the history and culture of his native state's low country, has chronicled with impressive documentation the rise and fall of a way of life that was close to the plantation legend of the Old South. Although he has written a final chapter on modern Georgetown that reads like a chamber of commerce brochure, and although he ignores the impact of the civil rights movement on the county, he has made a solid contribution to the history of the Southern plantation. In him the great rice planters of the Waccamaw, the Pee Dee, and the North Santee have found their elegist.

NORMAN D. BROWN
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WILLIAM CHAZANOF. *Joseph Ellicott and the Holland Land Company: The Opening of Western New York.* (A New York State Study.) [Syracuse:] Syracuse University Press. 1970. Pp. ix, 240. \$9.00.

As this volume demonstrates, Joseph Ellicott was a "pivotal figure" in the early settlement of western New York. He first surveyed the Holland Land Company's three-million-acre tract west of the Genesee River and then was made resident-agent in 1800, a post he held for twenty years.

From his office at Batavia, Ellicott sold lands, helped to organize towns and counties, planned internal improvements, and exercised a pervasive political influence. He was, writes Professor Chazanof, "at the core of the power structure of western New York." Endicott's first loyalty was to the interests of the Dutch investors as he sought to hold down taxes on company lands, to endure the difficulties caused by the War of 1812, and to guide the course of the Bank of Niagara at Buffalo. Concerned primarily with the value of company lands, he supported the building of the Erie Canal, advising on the selection of the route and serving as a canal commissioner from 1816 to 1818.

This book gives us fresh information on

Ellicott's personal, family, and company relationships, his influence in the development of the western New York frontier, and his activities as a Jeffersonian, Clintonian, and Bucktail in New York politics. Extensive research was done in manuscripts and documents both in the United States and Holland, and eight maps add clarity to the text. Nonetheless, the volume is poorly written, especially in its redundancy. It is burdened by excessive and sometimes ill-chosen quotations from the letters of Ellicott and his superior in Philadelphia, Paul Busti, causing the narrative to move slowly. A laudable effort has been made to place western New York in larger context, but much of this material seems extraneous. The chapter on the Bank of Niagara, for example, opens with a discussion of banking in Holland and England, moves to Philadelphia and New York, and then begins the account of banking in western New York. In his chapter on "The Grand Canal" the author adds important detail to show Ellicott's part in the origins of the Erie Canal, but he has failed in some instances to recast published material into his own words.

In this study, unfortunately, the results of extensive research on a significant subject have been marred by serious deficiencies of style that might have been helped by more careful editing.

RONALD E. SHAW
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Oxford, Ohio

ERNEST R. SANDEEN. *The Roots of Fundamentalism: British and American Millenarianism, 1800-1930.* Chicago: University of Chicago Press. 1970. Pp. xix, 328. \$12.00.

Professor Sandeen has attempted a reinterpretation of the history of fundamentalism. His major point is that fundamentalism must be studied as a total phenomenon stretching in time from the French Revolution to the present, and cannot be explained, as some writers have tried to do, in terms of this century alone or the 1920s in particular. The author also points out that fundamentalism ought not to be defined dogmatically in terms of the five-point creed adopted by the Presbyterian General Assembly in 1910; a much more relevant dogmatic basis can be found in the so-called Niagara Creed of 1878. In this Niagara Creed

Professor Sandeen finds what he regards as the clue to the continuity of the fundamentalist movement—a millenarian element. Millenarianism, which entails the view that human history can only become more corrupt and abominable until the coming of Christ, was stimulated by the French Revolution and grew in strength until the end of the nineteenth century. Here Professor Sandeen rightly underlines the influence of the formidable J. N. Darby, who was able to persuade many American Protestants to accept his millenarian outlook without ever being able to separate them from their own denominations. Fundamentalism spread a loose structure of its own across the existing denominations. The leaders of the movement were as urban and bourgeois as the typical liberal Protestant: there is no question of a simple conflict between rural and urban world views. In the 1920s, however, the penetrative power of fundamentalism collapsed quite suddenly, isolating the more extreme millenarians in what proved an insignificant schism.

Professor Sandeen is widely read in his subject, and his opinions are clear and sensible. It seems a pity that having introduced his subject by giving a good outline of the revival of British millenarianism in the early nineteenth century, he does not return to the British parallel in the twentieth century: this omission partly explains why he fails to give a convincing explanation of the loss of influence of millenarianism in the 1920s. One cannot describe this kind of fringe Protestantism in terms of ideology alone: one has to ask not only what do people believe, but why do these people believe it? This said, it remains true that Professor Sandeen has provided valuable information on one of the obscurer aspects of recent church history.

JOHN KENT

University of Bristol

J. MAULDIN LESESNE. *The Bank of the State of South Carolina: A General and Political History*. (Tricentennial Studies, Number 2.) Columbia: University of South Carolina for the South Carolina Tricentennial Commission. 1970. Pp. ix, 211. \$6.95.

For fifty-three years, from 1812 to 1865, what many historians have described as the agrarian, precapitalist state of South Carolina owned

and operated an effective and prosperous banking corporation, the Bank of the State of South Carolina, that acted as the fiscal agent of the state treasury, provided a sound and elastic local currency including small-notes for use as change, and through loans and subscriptions to stock encouraged the development of canals, railroads, and manufactures. It was required by law each year to devote part of its capital to loans to planters and farmers on ten-year terms, but most of its loans, like those of the privately owned banks, were to merchants in the towns and cities. It also profited largely from the purchase and sale of bills of exchange, and for this purpose as well as to provide for the payment of interest and principal of the state debt it had established and mutually profitable relations with banks and merchants in New York and London.

Not all South Carolinians approved of this direct participation by the state in the business of banking, and for a time in the late 1830s and early 1840s, the bank was under almost continuous political attack. Its opponents objected to its seemingly unprofitable investment in railroad stock, to its policy of renewing loans when, because of the generally disturbed financial conditions, its borrowers were unable to pay, and in particular to a very large loan to an iron manufacturer, the Nesbitt Manufacturing Company. This company had been incorporated by the state with the deliberate purpose of "the training and qualifying slaves for the manufacture; a diminished price for, and a better quality of iron, and above all a place where arms and munitions can be manufactured to defend us if our liberties and country are ever in danger."

The return of prosperity in the late 1840s enabled the bank to survive the political attack, and during the Civil War, through skillful management, it spared South Carolina many of the financial difficulties that hindered the war effort in the other seceding states as well as in the North. In the future historians, when describing antebellum South Carolina, will be forced to consider the implications of this previously untold story even though the author has not called attention to them. He confined his attention, as his subtitle indicates, to the story of the bank as recorded in the public documents (the account books, correspondence, and

other records have disappeared), and does not concern himself with anything beyond his immediate subject. But he has written an important and significant book and one that may drastically alter the accepted interpretation of the nature of the South Carolina society.

THOMAS P. GOVAN
University of Oregon

ELMER LOUIS KAYSER. *Bricks without Straw: The Evolution of George Washington University*. New York: Appleton-Century-Crofts. 1970. Pp. xiv, 352. \$9.95.

THOMAS D. CLARK. *Indiana University: Mid-western Pioneer. Volume 1, The Early Years*. Bloomington: Indiana University Press. 1970. Pp. xvii, 371. \$10.00.

WILLIAM A. KINNISON. *Building Sullivant's Pyramid: An Administrative History of the Ohio State University, 1870-1907*. (Published in observance of the 1970 Centennial of the Ohio State University.) [Columbus:] Ohio State University Press. 1970. Pp. xii, 225. \$8.00.

LOUIS G. GEIGER. *Voluntary Accreditation: A History of the North Central Association, 1945-1970*. Menasha, Wis.: North Central Association of Colleges and Secondary Schools. 1970. Pp. xxiv, 199. \$5.00.

The authors of these histories were well equipped for their assignments. Elmer Louis Kayser has wrestled successfully with finances during his long term as treasurer of the American Historical Association; he has also been identified with George Washington University for over half a century as student, professor of history, and administrator. Thomas D. Clark, on the other hand, approached his task without previous association with Indiana University but with rich experience as a university teacher, trustee, and productive scholar in American social history. Professor Louis G. Geiger is the author of a commendable history of the University of North Dakota, while William A. Kinnison is a qualified historian with administrative experience.

As one would expect, all these histories rest on thorough and unrestricted research in the official records of the institutions with which they are concerned. These histories of George Washington (Columbian College and Columbian University until 1904), Indiana, and to a somewhat lesser extent by virtue of its focus

on administrative history, Ohio State, also profit from the use of manuscript records of trustees, presidents, faculty, the student organs, and the newspaper press. Wide-ranging and excellent illustrations enhance the interest of the Kayser and Clark volumes.

The three university histories present a clear picture of the distinguishing characteristics of each: the emergence of the Columbian University as one of our first urban universities serving government employees with opportunities for further study and, in the case of several nationally known scientists, for part-time instruction; Indiana University, handicapped by the apathy, anti-intellectualism, and sectarian bigotry of Hoosier culture and by the denial of an opportunity to serve the farming population directly when the decision was made to establish Purdue as the Morrill land-grant institution; and Ohio State, slower to get under way than its neighbors and stymied by conflicting regional, political, religious, and educational interests. At the same time, in varying degrees, these histories commendably relate the institutions to the changing currents of higher education and to the immediate and larger communities in which they functioned. Without sacrificing local color and characteristics, including the physical plant and campus, the teaching staff, and the student bodies, the historians of the three universities transcend the purely parochial and thus contribute to a better understanding of American higher education in its broader sweep.

No clear consensus of what kind of an institution was wanted existed in the minds of the founders and early directors of these institutions. The Columbian College owed its origin to a religious impulse, though it was also hoped that with a congressional charter and the patronage of leading public figures in the federal capital it might become the great national university that George Washington had envisioned. Indiana College was in the main conceived as an institution for developing an elite of Christian gentlemen trained in the tradition of the classical seminary. Though feeble attempts were made to meet the demand for utilitarian services through instruction in law and engineering, it long remained a small liberal arts college. Ohio State was the result of the political ambition of Benjamin Wade

to further divide Ohio Democrats by associating the Republican party with the acceptance of the state's portion of the proposed Morrill land grant. Some of its founders and directors wanted a simple agricultural school, others an industrial training institution, others a combination of the two, and still others, among whom Joseph Sullivant was especially important, a great modern university modeled on Cornell. In the end Rutherford B. Hayes, acting as mediator, effected a compromise between these conflicting objectives, which accounted for much of the character of the institution.

All the authors properly stress the matter of control. Except for a brief period when the Columbian University was dominated by Baptists, ambivalence marked its government for three quarters of a century. It received city lots from Congress and was subject to investigation by the attorney general. Ohio State was dominated by a strong board of trustees that allowed presidents and faculty little influence. Indiana was also subject to an ever watchful and interfering board of trustees that in turn was under some political influence.

In all three cases the miracle is that in view of the meager and uncertain support these institutions survived at all. The financial problems in each instance make sad reading and suggest, perhaps, the need for qualifying the generally accepted idea of American enthusiasm for education. The Baptists did next to nothing for the Columbian College and University and the only notable donor was W. W. Corcoran. Crisis after crisis marked its fortunes; it existed under a burden of staggering debt with the wolf often halfway in the door. The initial land grants of Indiana were badly handled. Ohio State's land grant yielded far less than might have been the case under different management. Not until 1891 could it count on regular legislative support, a support lessened by the insistent claims of Ohio University and Miami University. In the case of both Ohio and Indiana public support was delayed and restricted by the opposition of the sectarian colleges: the image of a godless institution flourished despite the fact that Indiana's first six presidents were clergymen and that Ohio State's trustees insisted on compulsory chapel long after it was given up in other state universities.

All three institutions were both fortunate and unfortunate in their presidents. The Columbian College had, to be sure, loyal and devoted heads, but it was not until the time of James Clark Welling in the late nineteenth century that it enjoyed outstanding leadership. Indiana's first president, Andrew Wylie, fixed his conception of a classical and Christian education on Indiana and did little to develop cordial relations with the public. It was not until 1884, when David Starr Jordan became president, that the institution began to feel the impact of the new nationwide emphasis on the natural and social sciences. Even so, in Professor Clark's judgment, Indiana was not to achieve full university status until the twentieth century: we wait eagerly his concluding volume for an explanation of how Indiana achieved its distinction. At least until 1907, the terminal date of Kinnison's history, Ohio State did not have the good fortune in its leadership that tardily came to Indiana and that Michigan and Wisconsin enjoyed.

At both Indiana and Ohio State the active role of students seems to have been more consequential than in many institutions. In 1864 Indiana students, for example, won the first victory for academic freedom by insisting on their right to hear outside speakers. It is also clear that in view of the routine and generally uninspired classroom instruction the literary societies in all three institutions performed an important student-directed educational experience.

Professor Geiger observes that most historians of higher education pay little attention to the efforts of regional accrediting agencies to improve standards both in institutions of higher learning and in the secondary schools and to coordinate the two. Thus the history of the later years of the North Central Association is a welcome addition to educational history. Its story, while replete with accounts of endless reports, surveys, and conferences, nevertheless succeeds in bringing to the fore the larger issues involved in the characteristically American effort to achieve by means of a voluntary association the upholding and improvement of academic standards with recognition of the value of flexibility and innovation. As such it is an important contribution to an understanding of the frustrations and achievements in the

efforts to improve the educational process in a period truly revolutionary by reason of the problems created by war, a vast increase in federal support, a new technology, urban decay, racial tension, and the revolt of a considerable part of the student population.

MERLE CURTI

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Madison*

WILLIAM SEALE. *Sam Houston's Wife: A Biography of Margaret Lea Houston*. Norman: University of Oklahoma Press. 1970. Pp. xv, 287. \$6.95.

More than fifty biographies have been written about Sam Houston, but this is the first full-scale treatment of the mother of his eight children who lived to maturity. The author makes a heroic effort to treat Margaret Lea Houston as an individual and with sympathetic understanding, but even though she possessed many remarkable qualities her place in history obviously rests solely upon her long and devoted relationship to the most famous Texan of them all.

Margaret Lea might well have been the least interesting of Houston's three wives, but the first two are shrouded in considerable mystery. Eighteen-year-old Eliza Allen married the governor of Tennessee in 1829. Three months later she packed her belongings and went home to her parents and never revealed the details of the separation. Neither did Houston, who subsequently resigned from office and for the next several months sought refuge among the Cherokees in Oklahoma, temporarily taking an Indian wife named Diana Rogers Gentry and acquiring the nickname "Big Drunk."

Like many other restless frontiersmen of the period Houston soon moved on to Texas. Four years later, at the battle of San Jacinto, he led a ragtag army against General Santa Anna's Mexican forces and defeated them decisively. From 1836 until his death twenty-seven years later Houston's name was virtually synonymous with the history of Texas. Meanwhile he journeyed to New Orleans to obtain medical treatment for injuries received at San Jacinto. There he met Alabama-born Margaret Lea, twenty-six years his junior. The two were married a short time later in the face of strong opposition from family and friends on both sides. The

Leas were staunch Baptists, while the twice-married Houston was a known alcoholic and more or less contemptuous of organized religion. Somehow the union survived and Houston's affection for his young wife remained genuine, even if his ultimate conversion to the Baptist Church did not.

The central theme of Seale's biography is Margaret Lea's unceasing devotion to her controversial husband and to their children. The picture of her that inadvertently comes through is that of a frail, sickly, religious fanatic who suffered from melancholia and who could never come to terms with the Texas wilderness. "To day while reading my precious Bible," she wrote to Senator Houston in Washington in 1848, "the words seemed so sweet that with childlike fondness involuntarily I pressed it to my heart and buried my weeping eyes in its hallowed pages." Yet this same individual on one occasion took a heavy cowhide strap to a nineteen-year-old servant girl who had mistreated one of the Houston children and beat her unmercifully. The girl carried scars for several months, but according to her mistress she seemed happier and more cheerful after the lacerating than "I ever saw her."

The present biography is well written, copiously documented, and attractively produced.

W. EUGENE HOLLON

University of Toledo

JOHN FRANCIS McDERMOTT, editor. *Travelers on the Western Frontier*. Urbana: University of Illinois Press. 1970. Pp. xii, 351. \$10.95.

The American West as viewed by both foreign and domestic travelers is a subject of continuing interest to historians. This volume is the published result of a Conference on Travelers on the Western Frontier held at Southern Illinois University, Edwardsville, in February 1968. It is a "second helping" that followed a similar conference held at the same place in November 1965, which resulted in *The Frontier Re-examined*, also edited for publication by Professor McDermott.

Travelers on the Western Frontier is principally bibliographical, a volume in which the participants have drawn attention to hitherto unexploited manuscript resources or to neg-

lected sources. Following an introductory and rather general account of Missouri River travel diaries by Professor McDermott are discussions of sources at such places as Yale, by Archibald Hanna, Jr.; at the Bancroft Library, by Dale Morgan; at the Newberry Library, by Dwight L. Smith; and in the Reports of the United States Congress, by Herman R. Friis.

Newspaper and magazine accounts, frequently overlooked by scholars, comprise another principal section of the volume. The *Spirit of the Times*, published in New York between 1831 and 1856, was examined by John T. Flanagan. John Porter Bloom similarly considered *The American Turf Register and Sporting Magazine* for the years 1829-44, gleaning from it references pertaining to the West. *Weekly Reveille*, a St. Louis publication (1844-50) that looked westward from the "gateway," was the subject of Nickolas Joost's contribution, while Nicholas Canaday, Jr. perused the New Orleans *Picayune* (1836-60) for similar material.

Of more general interest were offerings on Western boatmen by Richard E. Oglesby, travelers' views on Indians by Donald D. Miner, the experiences of a French scientist among the Comanches of Texas by John C. Ewers, and William B. Baker's study of early roads west of St. Louis.

As most program chairmen know, it is difficult to put together a well-knit offering, and it is even harder to find a group whose contributions are so uniformly good that all, or even most, are publishable. Here Professor McDermott appears to have been both lucky and shrewd, for his participants are truly professionals, an impressive number of whom have national reputations in the field.

Therefore, the publication of McDermott's 1968 conference papers will find a useful place in the bibliography of the field and will be particularly helpful to younger scholars looking for new approaches to a field that has been pretty well worked in the past generation.

ROBERT G. ATHEARN
University of Colorado

JAMES ROGER SHARP. *The Jacksonians versus the Banks: Politics in the States after the Panic of 1837*. New York: Columbia University Press. 1970. Pp. xii, 392. \$12.50.

The Democratic war against banks did not end

with Andrew Jackson's presidency. When the panic of 1837 rekindled antibank sentiment in the party, Martin Van Buren called for the Independent Treasury, and in the states Democrats launched a variety of assaults against the banking system.

Based on immense research, James R. Sharp's informative new study surveys these state-level battles on a nationwide scale, comparing the struggles in the Southwest, Northwest, Southeast, and Northeast. Although Sharp concentrates on the first three sections, he effectively refutes the idea that New York politics were typical of the Northeast, let alone the rest of the nation. He analyzes intensively the banking issue and Whig and Democratic voting constituencies in three states—Mississippi, Ohio, and Virginia—and then briefly but skillfully surveys the struggles in other states after 1837.

While the Whigs were united in defense of private banks, Democrats divided into at least three factions: probank or soft-money conservatives who recognized the necessity of banks for economic development; political brokers like Thomas Ritchie who cared more about maintaining party unity than about the bank issue; and antibank, hard-money men who constituted the majority of the party and who shaped its ideology and policies after 1837. Rejecting the entrepreneurial thesis that antibank sentiment reflected a desire to democratize business, Sharp argues persuasively that it represented an egalitarian abhorrence of the power and privilege of banks. In the South and West, moreover, agrarian hostility to the commercialization of the economy and nostalgia for a simpler society contributed to the detestation of banks and paper money.

The intensity of antibank sentiment varied from area to area. Antibank forces were most dominant and radical in the West where the depression was most severe and where banks were relatively new. There they eventually attempted to destroy banks and abolish paper money altogether, succeeding temporarily in the Southwest. In the eastern sections, where the depression was less severe and where banks were an integral part of the economy, Democrats either favored state-owned banks against private banks or demanded an end to special charters and state-imposed reforms of banking practices.

Sharp contends that the contrasting views about banks derived essentially from the different experiences of Whig and Democratic voters in the changing economy. Recognizing that patterns varied in different regions within states and that some noneconomic factors influenced voting, he concludes that probusiness Whigs came from wealthier constituencies "that were fully participating in the market economy" (p. 325), while hard-money Democratic strength came from "areas outside the market economy and ones of declining wealth, power, and prestige" (p. 325), from Northeastern cities, and from Germans. While this conclusion is logical, Sharp's analysis raises certain methodological problems. To identify party constituencies, Sharp correlates the county votes for President, 1836-44, with indexes of average wealth and slaveholding by county. He admits the hazards of working on the county level, but his assertions about Ohio and Virginia are strained because he relies on correlation coefficients of .43 and .20 to prove that Democrats came from poorer areas. While Sharp's explanations of discrepancies in these states are intelligent, one still wonders who voted for a minority party in a county if that county's relative wealth determined popular voting behavior. Answering such questions is always difficult when using aggregate voting returns, but Sharp may well have had stronger evidence had he gone below the county level and discovered that the parties drew from richer and poorer groups within counties. More troubling, Sharp tends to confuse Democratic strength with hard-money strength. He has identified Democratic constituencies in presidential elections that had no necessary connection with the money issue, but he argues that because Democratic state legislators supported hard money those constituencies did. Indeed, Sharp seems to believe people voted Democratic primarily because of their hard-money views. He may well be correct, but his analysis does not prove it. It is equally possible that Democratic voters supported antibank legislators simply because they were Democrats, not for ideological reasons.

If Sharp's voting analysis does not completely support his thesis, his traditional evidence suggests that he is on the right track. In addition, because of his commendable effort to treat state-level politics nationally, his book is a welcome

and important contribution to the history of the Middle Period.

MICHAEL F. HOLT
Yale University

JAMES C. CURTIS. *The Fox at Bay: Martin Van Buren and the Presidency, 1837-1841*. Lexington: University Press of Kentucky. 1970. Pp. xi, 233. \$8.50.

Historians have paid less attention to Martin Van Buren's single presidential term than to his years as party builder, presidential adviser, and defeated candidate. Yet in his presidency some important ingredients were added to the structure and issues of American politics. James Curtis' useful study fleshes out the political story of those years. His emphasis is on the debilitating impact on the Democratic party of a number of episodes, particularly the long battle over the Independent Treasury and the crisis in America's relations with her neighbors.

Curtis' theme is the restiveness of many elements in the Democratic coalition as Jackson's term ended, the subsequent revolt of some of them, and the divided party's consequent defeat in 1840. His argument rests on an interpretation of the party as primarily a centralized, nonnational "coalition of state interests, bound together by agreement on the need to limit federal activity and allow local diversity." Thus any party leader who stressed national policies as against peculiar state interests, as Van Buren was forced to do after 1837, was asking for internal party difficulties. Although Van Buren appears as a competent, even a strong leader, his situation grew intolerable as his national fiscal policy alienated important local Democratic groups.

Although he argues with moderation and skill, Curtis is not always convincing. He never clearly resolves how much the Democrats' failure in 1840 stemmed from their internal problems as against the destructive voter revolt provoked by the depression. Nor is his picture of the Democratic coalition complete; one can view the party structure of the period in different terms. The late 1830s were years of an increasingly partisan ideological congruence as well as of a developing and often effective national party structure. In fiscal policy, for example, the recent studies of Roger Sharp and James McFaul suggest that a consensus emerged among

the Democrats in this period, with relatively few resistant to the policies pursued by the administration. Curtis' evidence on this point is drawn primarily from New York and Virginia where serious discord existed, but we learn little about the situation in other states. Such nationalizing tendencies as existed importantly affected the Democrats despite pressures to the contrary. In focusing on partisan weakness and dissension Curtis has helped us understand one set of elements in a complex political culture.

JOEL H. SILBEY
Cornell University

WILLIAM G. MCLOUGHLIN, *The Meaning of Henry Ward Beecher: An Essay on the Shifting Values of Mid-Victorian America, 1840-1870*. New York: Alfred A. Knopf. 1970. Pp. xiii, 275, viii. \$7.95.

The title and subtitle of this book are important in reverse order; it is primarily "an essay on the shifting values of mid-Victorian America, 1840-1870" as exemplified in the career of Henry Ward Beecher. The thesis seems to be that Beecher, who "was such an intellectual sponge that he picked up every cliché and catch-phrase of the moment and spewed them out at random," reflected as an unclouded mirror the changing scene in which he lived. As such he was an interpreter of the contemporary, a "pompous, vain, bombastic, sentimental"—"popularizer [and] prophet of the ordinary." The question is "how and why such a man achieved such fame."

Most of us academics, who are accustomed to have students pay to listen to us in small classes, are so uneasy in the presence of any preacher, lay or clerical, whose message attracts a large number of people that we automatically use the word "popularizer" in a pejorative sense. McLoughlin is no exception. Hence, while he proposes as a chief purpose of the study an explanation of "why Beecher seemed to most middle-class, church-going Americans of his day a very important man," he cannot fully accept the simple answer he suggests—that Beecher "did provide spiritual nurture and sustenance of a necessary kind to a very large number of Americans," and "developed various lines of argument for helping his congregation cope with all of its problems."

To write of any of the Beechers is a formidable task, and of Henry Ward most difficult of all—this very prolific writer who "made a virtue out of attacking theological systems" and who, with Emerson, elevated self-contradiction into a principle a century before the neo-orthodox had baptized it with the name of "paradox." "How," asks McLoughlin, "is anyone to make any sense out of a man like this?" He does it as everyone else has, primarily by nicely trimming out of the amorphous mass of the man those elements that contemporary historians deem important about the era—in this case "the shifting values."

This means that this book is not a biography of Beecher, a task McLoughlin summarily dismisses without explanation as "if not impossible, at least not worth the effort." Rather it is a study of "what is significant" about Beecher. At this point McLoughlin overtly rejects the psychoanalytic approach of Erik Erikson, which is based on the hypothesis that the potentially popular leader is so much a part of his times that the resolution of his personal problems has almost universal appeal in his period. In claiming "only to show that [Beecher's] solutions were adapted to his age," however, McLoughlin cannot escape the implication that Beecher's "problems were those of his age" by simply denying any attempt to show that they were.

Next McLoughlin attempts to put Beecher "back in the context where [McLoughlin thinks] he properly belongs . . . the period from about 1840 to 1870." This justifies "seldom if ever" quoting anything Beecher "said or wrote in his very prolific years after 1870" when "he went on to even greater heights of popularity." And McLoughlin thus brushes aside the historians who "have seen [Beecher] as a spokesman for the Gilded Age" with the remark that Beecher "was a representative of a generation which preceded," a "child of an earlier era [who] was not equipped to cope with" the "major new problems" of the burgeoning era of industrialism and urbanism and who consequently "made ridiculous statements" that when quoted "make him appear a moral idiot." This is not a convincing treatment of the last seventeen years of Beecher's life when he reached those "heights of popularity" presumably because "his solutions" continued to be "adapted to his age." Nor does characterizing Beecher following 1870

as "an intellectual sponge" seem adequate to describe the quality of the man who produced the Yale Lectures on preaching (1872-74), founded the *Christian Union* (later *The Outlook*) in 1870, and publicly endorsed Herbert Spencer in 1878 and Charles Darwin in 1882.

If, as McLoughlin says, most scholars have been wrong in some academic sense in considering the Yale Lectures Beecher's "most mature and valuable work," his explanation of why, for his purpose—assessing the meaning of Beecher—they may be ignored is not convincing. For there can be little doubt that they have had a tremendous influence on both the content and style of preaching from Protestant pulpits down to the present.

Finally McLoughlin trims Beecher to manageable size and explains why the period after 1870 may be largely ignored by contending that it is the "ideas, first laid out in an orderly fashion in *Norwood* [1867], which constitute whatever claim Beecher has to historical importance." The force of this contention is weakened by McLoughlin's criticism on the previous page of scholars who have assumed that Beecher's "ideas did not change very much once he had acquired his reputation," presumably by 1870. Back of this contention one senses the typically academic assumption that ideas orderly laid out are the only matters of "historical importance"—the only basis for assessing the "meaning" of a character.

These comments are not to denigrate McLoughlin's admirable achievement in making "sense out of a man like this," but only to remind the reader that in the same sense that we commonly refer to "Herndon's Lincoln," this is "McLoughlin's Beecher."

SIDNEY E. MEAD
University of Iowa

PHYLLIS FLANDERS DORSET. *The New Eldorado: The Story of Colorado's Gold and Silver Rushes*. [New York:] Macmillan Company. 1970. Pp. viii, 434. \$12.50.

As one picks up this book, looks at the dust jacket, and reads the blurb and the acknowledgments he expects to find a lively, irresponsible collection of stories that are mostly fiction. As he proceeds, however, he happily finds that the author has indeed done very extensive reading in secondary sources and has been

rather meticulous in her search for and utilization of facts. If he looks at the notes, even in their abbreviated and nonorthodox form, he gains faith in the authenticity of the narrative.

It is not a balanced history of Colorado; indeed it does not pretend to be. It is primarily the story of the search and struggle for gold—enumerating crimes, describing prostitution, and detailing political maneuverings, the activities of bunco artists, and the frailties of human nature. The writing emphasizes the dramatic and spectacular. For example: "When he drew opposite Uncle Dick's saloon, Rooker fired both barrels at the gambler. The sound bounced back and forth between the buildings as O'Neil, spurting blood bright red in the sunlight from thirteen holes in his gut, crumpled into the street"; or "From White Antelope's body Harry Richmond and Hal Sayre triumphantly cut off the scalp and testicles. From the bodies of squaws, soldiers sliced off breasts and wore them rakishly atop their hats or stretched over saddlebows"; or "Although only two blocks away from these grandiose structures flourished Sodoms to match any mankind had yet put together."

Although the events and incidents are generally factual the resulting picture is hardly true. The historian's craft calls for authentic facts to be sure, but it also demands such a selection of facts as will present an authentic general picture. If the author is endeavoring to present such a picture of early Colorado, she fails; but if her purpose is to paint dramatic scenes, portray primarily the seamy side of life, and give lively entertainment, she succeeds.

LEROY R. HAFEN
Brigham Young University

BRAY HAMMOND. *Sovereignty and an Empty Purse: Banks and Politics in the Civil War*. Princeton: Princeton University Press. 1970. Pp. ix, 400. \$10.00.

In 1957 Mr. Hammond established his high prestige as a specialist in the difficult field of economic history with his 771-page opus, the Pulitzer Prize-winning *Banks and Politics in America from the Revolution to the Civil War*. He followed this in 1961 with "The North's Empty Purse" (*AHR*, 67 [1961-62]: 1-19), a severely compressed distillation of his research on the congressional struggle over what became the Legal Tender Act of February

25, 1862. The outcome of that struggle hinged upon persuading Congress to implement enough national sovereignty to permit issuance of federal currency.

The question of sovereignty perforce continually confronted members of the Thirty-seventh Congress as they wrestled with such allied national problems as further issues of money, federal revenue, and a banking system. So Mr. Hammond yearned to achieve a second opus, centering upon the sovereignty issue. But the thoroughness ingrained in his twenty years on the research, documentary, and diplomatic staffs of the Federal Reserve Bank in Washington kept its grip upon him. By August of 1964 he was confessing to me that he must constrict the span of his researches; but as late as March 1968 he was still avidly pursuing elusive data. When he died the following July at the age of eighty-two, he left behind a "just-finished manuscript" for which Melitta de Kern Hammond performed the "last rites"—correlations, citations, and so forth.

This final book is mainly devoted to the revenue, monetary, and banking measures of the Thirty-seventh Congress, enacted while the nation struggled with the unfamiliar problems of the first two years of the Civil War. As legislation emerged from the welter of conflicting evidence Hammond treated the data as a challenge to his powers of analysis, recording the details exhaustively. At an early juncture he regretfully explained that events must be "told either in detail too tedious for most readers or with a severe brevity that blurs and confuses the facts. I have chosen to be tedious" (p. 51).

His microscopic analysis is early displayed in his thirty-page dissection of conflicting evidence on an obscure but important amendment to a supplementary loan act that went on the statute books August 5, 1861. Therein he concluded that Secretary Chase, a master of verbose and opaque prose, may have wordily altered an amendment that originally authorized what its sponsors intended so that it actually authorized what he intended; or, that it reached him authorizing what he intended and the opposite of what its sponsors intended. "I do not see how the second alternative can be preferred to the first. . . . The episode may be taken to show how much better it is to do

things quietly and obscurely than with a lot of noise and illumination" (p. 92).

Biographers can find here details on inconsistencies common among legislators; but Hammond tempers his exposés with humane choice of words, whether he refers to Chase (whom he does not admire) or to Sherman, whom he oftentimes portrays as the far-seeing statesman beseeching myopic members to see reality. It is unfortunate that we can never have a Hammond analysis of the less-statesmanlike Sherman of some later episodes. Comparing these two on state bank currency, for example, Hammond finds Chase "monstrously polite to the state banks" while "Sherman hit the banks hard, in plain words emphasizing evils" (pp. 300-01). Altogether, the Hammondian handling of economic politics, free of veneration for prior analyzes, warns historians with the temerity to try to deal with this subject that they can ignore Hammond's presentation only at the risk of their ultimate discomfiture.

JEANNETTE P. NICHOLS

University of Pennsylvania

DAVID DONALD. *Charles Sumner and the Rights of Man*. New York: Alfred A. Knopf. 1970. Pp. xxiv, 595, xxxix. \$15.00.

With this publication David Donald completes his study of Charles Sumner, a study that is one of the masterworks of American biography and one in which thorough scholarship is illumined with keen insight and literary grace. The second volume is longer than the first by about fifty per cent although it covers only the last thirteen years of Sumner's life, from 1861 to 1874. Yet these proportions are easily justified. In the antebellum Senate Sumner's function was primarily expressive rather than operative, and he absented himself for some three and one-half years after the Brooks assault in 1856. Then, at the point where Donald's new book begins, Sumner came into power, not only as a leading figure in the triumphant Republican party but also as the remarkably influential chairman of the Senate Committee on Foreign Relations, a position that he held during three presidential administrations. Thus the story suddenly grows more complex, requiring treatment in greater detail. Sumner, the oracle of antislavery radicalism, now has serious respon-

sibilities that sometimes cramp his style but also open new fields to the play of his undeniable abilities and mountainous ego. He is drawn into important new relationships—with the president, the secretary of state, and the diplomatic corps. His behavior more often has immediate and definable consequences. Among other things the book is a fascinating account of what happens when an inveterate dissident becomes associated with the establishment.

The very nature of its material makes this second volume somewhat looser in structure and more difficult to read than the first. Donald's narrative moves back and forth between foreign affairs and the domestic issues of wartime and Reconstruction; between Washington politics and Massachusetts politics; between Sumner's public career and his private life, which included a brief, disastrous experiment in matrimony at the age of fifty-five. If the book has a dramatic climax analogous to the Brooks affair in the earlier work, it is the removal of Sumner from his committee chairmanship in 1871—an overthrow engineered, perhaps fittingly, by one of his alienated friends.

Sumner's ambivalent relations with Lincoln and his administration are skillfully detailed. In spite of temperamental differences the two men had need for each other's cooperation and much in common, Donald maintains, at the level of fundamental convictions. It appears that each man took comfort from the belief that he was managing the other, and they remained personally on terms of cordiality, partly because of the friendship that sprang up between the senator and Mary Lincoln. Sumner pressed Lincoln hard on the subjects of emancipation and Negro rights, but such pressure may not have been entirely unwelcome. Often a severe critic of the president but never an outright political enemy, he took no part in the anti-Lincoln maneuvers of Republican malcontents in 1864. Of course he regarded Lincoln as his inferior and treated him at times with condescension, but that was the fate of most men who crossed Charles Sumner's path.

Between Sumner and William H. Seward, on the other hand, there was nothing but distrust and hostility from the beginning. The secretary of state not only secured the appointment of a Sumner enemy, Charles Francis Adams, as min-

ister to Great Britain, but he successfully resisted Sumner's efforts to "take over control of American foreign policy." More than that, Seward quickly became associated with the conservative wing of the Republican party and exercised, in Sumner's view, a malign influence on administration policies concerning slavery and the Negro. Seward's loyalty to Andrew Johnson after Lincoln's death confirmed the suspicion that he had deserted the original principles of his party. Yet Sumner, after some preliminary misgivings, lent decisive support to Seward's greatest achievement, the purchase of Alaska.

Sumner probably reached the peak of his power in 1869. True, his relations with Ulysses S. Grant were never very good and grew steadily worse after he took the lead in frustrating the president's cherished plans for annexation of the Dominican Republic. But in Hamilton Fish he now had a friend as secretary of state. The new minister to Great Britain, John Lothrop Motley, was Sumner's choice and virtually his personal representative. At the same time Sumner won much popular support with a speech on the *Alabama* claims that held the British government responsible for indirect damages resulting from prolongation of the Civil War. Characteristically, however, he overplayed his hand and encouraged Motley to do likewise. When the latter was recalled Sumner quarreled with Fish, who became fed up to the point of pressing successfully for removal of Sumner from his chairmanship.

Some readers of this book may find that they have been told more about Sumner's influence on foreign affairs than they wanted to know and less about Reconstruction than they expected. There are two reasons for any such feeling of disappointment. First, Donald has more or less bypassed the interpretative historiography of Reconstruction, advancing no general thesis himself, making his own judgments on controversial points, and expressly disavowing any intention "to renew, much less to settle, old arguments among scholars." Second, Sumner's role in Reconstruction was curiously fragmentary and erratic. Oratorically he never ceased befriending the freedman, and for one brief period in the winter of 1869-70 he took command of Senate Radicals. But most of the

time he was too unpopular to attract much of a following. He was excluded from the Joint Committee on Reconstruction and saw his "bitterest enemy in the party," William P. Fessenden, appointed chairman of that powerful body. Possessing neither talent for legislation nor the capacity to accept compromise, he "introduced none of the major reconstruction acts . . . and frequently abstained from voting when they were finally adopted." In this respect, as Donald points out, he was strikingly different from Thaddeus Stevens, with whom his name is so often linked in Reconstruction history. Sumner contributed nothing but criticism to the Thirteenth Amendment; he blocked passage of the Fourteenth Amendment for a time; and he refused to support the Fifteenth Amendment on the ground that it was constitutionally unnecessary. His one important piece of legislation, the Civil Rights Act of 1875, was not passed until after his death.

Thus, in Reconstruction as in antebellum politics, Sumner's role seems to have been largely of an expressive nature. His doctrinaire mind limited his potential as a statesman, and too often he followed "that illogical logic that carries a premise to its utmost conclusions." But it is also true that in one of his convictions he was simply too far ahead of his age. "More than any of his political contemporaries," Donald writes, "Sumner realized that the future of American democracy depended upon the ability of the white and black races to live together in peace and equity."

Although Donald's first volume won high critical praise and the Pulitzer Prize, certain neo-abolitionist historians accused him of writing with determined hostility to his subject. But anyone interested in the whole truth must have mixed feelings about Sumner, as Donald plainly does. The problem is to strike a just balance between the man's admirable and repellent qualities. In this second distinguished volume Donald has tried hard to be fair and with greater success, I believe, than most biographers.

DON E. FEHRENBACHER
Stanford University

FELICE A. BONADIO. *North of Reconstruction: Ohio Politics, 1865-1870*. New York: New York University Press. 1970. Pp. xi, 204. \$8.95.

A monograph dealing with party politics at the state level during the five years after the Civil War, *North of Reconstruction* seems to be based on extensive research. The author is concerned with such matters as personal ambition and rivalry, faction, and patronage. He gives little attention to some of the currently popular approaches—ethnic and sociological politics, the place of the churches, ideological considerations, racism, and economic interests. He takes issue from time to time with several historians, chiefly Eric McKittrick and John and LaWanda Cox. His conclusions should be of interest.

Unfortunately the monograph has a large number of errors. For example Bonadio writes of Oran Follett and John Russell and identifies each as an ex-governor of Ohio (pp. 60, 158); neither was ever governor of Ohio. He writes that John McKinney "had been elected mayor of Miami" (p. 67); no such town or city exists in Ohio. He refers to a newspaper, which he calls "the *Portsmouth Democrat*," as evidence for the 1860s (p. 114); a newspaper with that title is reported in 1844 but not later. Perhaps it is the same as "the *Portsmouth Portage County Democrat*" of 1867 (p. 94), but this also seems erroneous since Portsmouth is in Scioto County; a newspaper with the latter name was published in Ravenna. A publication by Elizabeth Yager is cited as an article (p. 34) and soon after as a book (p. 35). An article by Edward Noyes is cited under an incorrect title and as a book (p. 107). Numerous other titles are incorrect, such as that of an article by Charles R. Wilson (p. 164). Personal and other proper names are often given in puzzling forms: Charles M. Destler (for Chester M. Destler?) and Frank Clement (for Frank Klement?). The single statistical table is confused—should the column headed "1886" be headed "1868"? Language is used inaccurately: "the Republican party won a sweeping national victory in 1860" (p. 134). Information and a quotation from printed sources are not to be found where the documentation says they may be seen (p. 34 n.7), and one of the two works cited there is given an incorrect title. The errors mentioned and others may be trivial individually. In cumulative effect their number, character, and variety are such as to destroy the confidence one needs to feel in the validity of information and in-

sights that are based on relatively inaccessible materials such as manuscripts and rare newspapers.

HARRY R. STEVENS
Ohio University

JACK D. FONER. *The United States Soldier between Two Wars: Army Life and Reforms, 1865-1898*. New York: Humanities Press. 1970. Pp. iv, 229. \$7.50.

The American soldier led a hard life in the late nineteenth century. In this book Jack D. Foner describes the problems of the military life and efforts to ameliorate these conditions. Between the Civil War and the Spanish-American War the regular, as a member of a small constabulary force that numbered some 25,000 throughout most of this era, helped carry out the federal government's will against Southerners, strikers, and Indians. Poorly and irregularly paid, the soldier had justifiable complaints about his quarters, food, and clothing. On occasion he might suffer from the whims of a martinet. If he got into trouble he was then caught up in the tangle of a confusing and inefficient legal system. Civilians viewed him with contempt while politicians periodically called for military budget reductions that would make his lot even more miserable. As one might expect, the desertion rate was very high. In the period 1867-95 more than ten per cent of the enlisted men deserted in each of fourteen years, and during four of those years the rate was higher than twenty-five per cent. Spurred by the desertion problem Secretary of War Redfield Proctor, Adjutant General John C. Kelton, and various other officers corrected many abuses and generally improved living conditions in the army.

Since Don Rickey, Jr. did an admirable work on the life of the soldier a few years ago—*Forty Miles a Day on Beans and Hay* (1963), Foner concentrates on certain aspects of the subject that Rickey did not develop—military justice and the reform movement as well as black regulars. Although there is a degree of overlapping the author is successful in supplementing Rickey's study. Yet, because of the nature of his approach, his book gives the impression of being a collection of scholarly papers. It is in fact his dissertation with little, if any, revision.

Although the author uses apt quotations and anecdotes throughout the book, he fails to bring out any of the several personalities who played crucial roles in the reform movement. Proctor, Kelton, and the others remain names. Foner did an impressive amount of research but his frequent carelessness with the names of peripheral characters is annoying. Despite its flaws, this interesting monograph is a definite contribution to the field of military social history.

EDWARD M. COFFMAN
*University of Wisconsin,
Madison*

RANDOLPH C. DOWNES. *The Rise of Warren Gamaliel Harding, 1865-1920*. [Columbus:] Ohio State University Press. 1970. Pp. x, 734. \$17.50.

For reasons defined more by the limitations of its subject than the industry of its author, this is a discouraging book. In his preface, Professor Downes notes that the opening of the main body of Harding Papers seven years ago enabled scholars "to attempt the preparation of an adequate biography." His version may or may not provoke further efforts. Arriving after Robert K. Murray's solid study of the Harding presidency, Francis Russell's lurid and engaging *The Shadow of Blooming Grove* (1968), and Andrew Sinclair's brief, spritely biographical sketch, Downes' study is respectable, scholarly, and anticlimactic. Grounded in a lengthy examination of Harding's political correspondence and contemporary press accounts, it carries matters forward to the election of 1920. The boundaries of the work are also fixed by a rigorous avoidance of Harding's private life and personal affairs. The result is political biography in the most narrow sense, a severely one-dimensional portrait. Had Harding's prepresidential public behavior been deeply interesting, or had the author probed the social landscape of Harding's Ohio environment with more methodological subtlety, a fresh political analysis might have helped our understanding of the remarkable ascent achieved by this unremarkable man. As it is, Harding emerges a flatter and more conventional figure than ever before. Density of detail, achieved mainly by generous quotation from his correspondence, editorials, and speeches, adds little to the depth of the portrait. Established images—the small-minded Marion

booster; the affable, rubbery, unimaginative state-house politician; the florid, uncertain senatorial conservative; the available second-choice presidential candidate—are all confirmed and amplified. Downes is not gentle with Harding, but the critique lacks tension and ignores paradox. Thus on page 140: “When he came to an issue he could not understand, Harding slaughtered it with eloquent misinterpretation.” Yet on page 256: “It is easy to see why his practical-minded fellow Americans, as they listened to his eloquent, obvious, and adjustable phrases, would eventually trust him with the highest office in the land.” As elsewhere the judgments are more pungent than incisive. Repeated themes and phrases endow the study with its primary conceptual framework: Harding’s broad rhetoric, calm dignity, and adjustability made him a successful mirage-maker in his quest for party unity amid the factional combat of Republican politics in Ohio, the Mother of Presidents. Despite his mistrust of most Progressive men and notions, only his stubborn reverence for tariff protection and his scorn for the direct primary qualified his ability to straddle and adapt. The author’s shrewdest chapters trace Harding’s reluctant but increasingly forceful try for the White House in 1920, an effort dictated at the outset by his anxiety about the impact of rival candidacies on his standing in Ohio politics. Almost half the book is devoted to the ensuing campaign, when a long, careful apprenticeship in the art of patching and smoothing finally paid off in an almost faultless mastery of issue manipulation and party unity. These were the obvious terms of Harding’s success. His latest and most dogged biographer has found them to be inescapable.

GEOFFREY BLODGETT
Oberlin College

JACK P. MADDEX, JR. *The Virginia Conservatives, 1867–1870: A Study in Reconstruction Politics*. Chapel Hill: University of North Carolina Press, 1970. Pp. xx, 328. \$8.50.

Twenty years ago C. Vann Woodward published his synthesizing *Origins of the New South, 1877–1913*; presently, in *The Virginia Conservatives, 1867–1870*, Jack P. Maddex, Jr. offers the Virginia Conservative party as an operating model of Woodward’s “Redeemer” phenomenon, while eschewing the term, and

delineates those adaptive Virginians who sought to adjust their state “to the tendencies of the bourgeois revolution of their time and to confer on it the benefits of industrial capitalism.” The author observes that the “new movement” of accommodation was initially led by moderate former Whigs who were shocked by the Reconstruction acts and who successfully courted president-elect Ulysses S. Grant. Younger Confederates, educated by war, enthralled by railroads, and motivated by a generational cleavage and a distrust of states’ rights platitudes, joined the self-proclaimed Conservatives and spread the party into southwestern Virginia and the Shenandoah Valley. This combination assuaged the bitterness of Reconstruction, which ended for Virginia in 1870.

To lower the voices of traditionalists and irreconcilables, Conservatives dampened politics, used railroad funds to finance campaigns, and generally ran the state free from fraud and mismanagement. Influenced by James S. Allen’s study of Reconstruction, Maddex implies that Conservatives feared a plebeian revolt more than black rule. No gross racial repression occurred, and the author incorporates Charles Wynes’s contention that race relations in Virginia were more stable and less violent than later. Faced with electoral defeat, however, Conservative candidates did resort to racist appeals, as in 1873, to offset the Radicals’ “issue of class.” Some leaders urged a Southern white-Northern Republican union, but Radical Republicanism and inherited loyalties re-established in 1876 the New York-Virginia Democratic axis.

In state policies Conservatives abandoned the reveries of an agrarian South and accepted an almost “eschatological vision of prosperity through capitalist development.” In banking as in railroading “*laissez faire* and northern economic penetration went hand in hand.” Public education was redirected to “practical” concerns to serve the new order. To be assured of Northern capital Conservatives pledged repayment of the state debt; the tax burden was shifted from businessmen to landowners. Uncharacteristically inflexible, Conservatives abandoned innovative reforms to balance the budget, and, under pressure from disgruntled plebeians, the national Greenback movement, and the ambitions of William Mahone, the party

disintegrated in 1879. Maddex concludes that Conservatives provided a temperate means of reunification and a pattern that subsequently evolved in the remainder of the South.

Blending earlier studies by James Douglas Smith, Robert R. Jones, and Charles C. Pearson, the author and his editors have reduced hundreds of pages of his doctoral dissertation to a graceful and pertinent text. Although Maddex neglects the influence of the pulpit and press, manuscript and secondary sources are so numerous and well integrated that the conceptual framework of *The Virginia Conservatives* will prove difficult for revisionists to overturn.

HENRY C. FERRELL, JR.
East Carolina University

ROBERT C. CARRIKER. *Fort Supply, Indian Territory: Frontier Outpost on the Plains*. Norman: University of Oklahoma Press. 1970. Pp. xv, 241. \$7.95.

Fort Supply was not a typical frontier military post, even though much of its career was comparatively prosaic. At the same time, it played some part in most of the significant developments occurring on the southern Plains over a quarter-century span. It was established in November 1868 as a base of operations for General Phil Sheridan's winter campaign and was retained, as Camp Supply, at the end of the campaign because its location was considered strategic and the wood, water, and natural forage necessary to sustain the post were available. Not until December 1878, following the Northern Cheyenne outbreak, did it become a permanent post, designated Fort Supply.

Located in what was then the Cherokee Outlet, Fort Supply was far removed from areas of settlement and major routes of travel. During the first dozen years of its existence its principal task, aside from normal escort service and occasional scouts, was to hold the Indians, notably the Cheyenne and Arapaho, within the limits assigned them and to protect them from whisky dealers, horse thieves, and others intent upon their exploitation. For the ensuing twelve years it played an important role in guarding the cattle trails across Indian country and continued its protection of Indian interests, now particularly from the cattlemen. The final duty

assigned the post was a part in policing the land rush when the Outlet was opened to settlement in 1893. Once the area was occupied Fort Supply was no longer deemed necessary and was abandoned in 1894.

Robert C. Carriker presents a straightforward biography of the post, and in considerable detail. At times, in fact, there is a plethora of names and minor incidents. These may be useful to readers in search of particular information, but they sometimes mar the flow of the narrative. Most of the book treats the military activities of the garrison, but one short chapter deals with life at the post itself and illustrates, within limits, some of the differences in the problems encountered by a post isolated from the settler. Apparently, Fort Supply was never called upon to contend with a "hog ranch" or with many of the conflicts with civilians that troubled so many posts. Extensive use has been made of the unusually complete archival material pertaining to Fort Supply. There are a number of well-chosen illustrations and useful maps.

ROBERT W. FRAZER
California State College,
Long Beach

WILLIAM LEONHARD TAYLOR. *A Productive Monopoly: The Effect of Railroad Control on New England Coastal Steamship Lines, 1870-1916*. Providence: Brown University Press. 1970. Pp. xvi, 323. \$10.00.

From Gustavus Myers to John Chamberlain to Gabriel Kolko, critics of the corporate order seldom have failed to denounce J. P. Morgan and his New Haven Railroad monopoly, simultaneously praising those (notably Louis Brandeis) who sounded the public alarm. In a study devoted largely to the New Haven's hold over New England rail and steamship transportation in the late nineteenth and early twentieth centuries, William Leonhard Taylor takes exception. The monopoly Brandeis attacked, Taylor contends, in reality "performed an essential function for New England."

Taylor demonstrates how necessary was the combined rail and coastal steamship network to New England's ability to compete in New York's markets. To a Boston merchant the celerity with which his goods could be shipped

was crucial; only through the combination of rail shipments to the ports of Fall River, Newport, or Providence, and thence to New York by direct water route, was it possible for him to maintain parity with his metropolitan competitors. The development of highly organized and efficient railroad monopolies over the Long Island Sound lines was the ultimate solution to express delivery and to mastery of New York's incredible inability to facilitate rail traffic. Ultimately, only the accelerated development of motor transport during World War I replaced this fifty-year arrangement.

Taylor's study is more than a treatise on monopolism. It contains enlightening sections on vessel construction, the beginnings of the coastal lines, the types of trade they catered to, labor conditions in the coastal service, and improvements and developments in the steamships themselves. The study is less successful in its ability to relate nationally. Little reference, for instance, is accorded similar situations outside New England. Such omissions suggest an unnatural importance, even uniqueness, for the New Haven monopoly. The fact is that by 1900 a nationwide amalgamation of coastal liners and railroads was almost a fixed thing. Witnesses to the U. S. Industrial Commission (1899-1902) made this abundantly clear, but Taylor uses this source for its sectional value only. Moreover, as others have pointed out, post-Civil War riverboat and coastal steamer competition only compounded serious regional handicaps for railroads, difficulties Taylor ignores. In another example, the depression of 1893, a catastrophic event for all railroads, and acknowledged as such by the author, is not demonstrated as relevant to this study. Finally, Taylor not only overlooks the 1913 decisions of the House investigation on shipping combinations, which corroborated his own promonopolistic findings, but also arrives at his overall conclusions without a single reference to major interpretations concerned with national, rather than regional, transportation and economics.

Nonetheless, as Taylor has viewed the New Haven, the faults of his own study are outweighed by its redeeming features. It is nicely written, it is useful, and it should be read.

JEFFREY J. SAFFORD
Montana State University

HUMBERT S. NELLI. *Italians in Chicago, 1880-1930: A Study in Ethnic Mobility*. (The Urban Life in America Series.) New York: Oxford University Press. 1970. Pp. xx, 300. \$8.50.

Significant numbers of historians are at long last turning their attention to the field of ethnic history and producing works beyond the filiopietistic stage, especially in Italian-American studies. Professor Nelli's book is one such effort. In clear prose and with well-turned phrases, he attempts to relate the Italian experience in Chicago within the larger context of the emergence of urban America. Aware of the ancestor-worship efforts of many nonprofessional historians who went before him, Nelli gives us a detached, impassionate, objective, and careful (perhaps at times too careful) study that scholars should find useful for many years to come.

Nelli's major theme is revealed in the subtitle: *A Study in Ethnic Mobility*. Utilizing real-estate documents in both City Hall and the county building, church records, city directories, school censuses, and other primary and manuscript materials, he plots the movement to the suburbs and other areas of Chicago attained by Italians who were spurred on by "economic success and desire for better living conditions." Indeed, when Nelli draws from real-estate records, parish records, and personal interviews, he gives us his most penetrating insights.

Also noteworthy is the author's recognition of the prejudice exhibited by the north Italians to the southerners, which contributed to the difficulties that the latter experienced in seeking acceptance in the social, political, and economic life of Chicago; his linking of some Italian immigrants with the cause of urban reforms; and his identifying an independent attitude in the political behavior of Italian-Americans (too many have viewed them simplistically as politically backward people). These findings are in accord with those of some scholars working on Italians in other cities.

There is much more to commend, but because of the relative newness of the field in which Nelli writes, one feels a special obligation to offer criticism that might be of benefit to the growing number of scholars concerned with ethnic studies.

Nelli's knowledge of the literature of the field and the latest research is impressive. Yet

some of the works closest to his area of interest are omitted in the footnotes or bibliography. In particular, his failure to cite the pioneer dissertation on Chicago's Italians by R. J. Vecoli is puzzling. Certainly, the reader interested in Chicago's Italians, especially in the area of labor, will want to use Vecoli's work to supplement Nelli's. Moreover, those familiar with Nelli's unpublished dissertation and his four articles will find little in the way of substantive change. While publishing books in this manner is not without precedent or value, the reader should be apprised when it happens. More important, I felt a lack of continuity and overall analysis.

Nelli speaks of the *padrone* system and community institutions as if they were unrelated: "Within the colony three institutions developed that sought principally to provide guidance and leadership for neighborhood residents. These were the benevolent societies, the Church, and the Italian-language press (the banker and padrone labor agent, who have been discussed elsewhere, concerned themselves entirely with financial profits and made no claims about contributing to group welfare or morale)" (p. 157).

Recently, some scholars have challenged the notion that Italian bankers and labor agents had only selfish motives. Moreover, whether or not they made claims about contributing to group welfare is not entirely relevant. The point is that they could and did do so. Equally important, Nelli agrees with the Dillingham Commission, which claimed that by 1911 "the boss system could be found 'only in a few isolated cases among Italians'" and asks "What caused this rapid decline of bossism?" (p. 63). He might also have asked where the bosses went and what they did if indeed their system was effectively silenced? In some other parts of the country *padroni* often became respectable leaders among the Italians and in the three institutions mentioned by Nelli.

Nelli speaks of a Father Mangone buying an old Protestant church and of his successor, a Father Lorenzoni, building a new one (p. 191). Where did the money come from? Who were the lay trustees who could raise the necessary finances? Who were the leaders of the benevolent societies? In some cities, former *padroni* were often found playing prominent roles in

building churches and organizing benevolent associations.

Nelli speaks of residential mobility. Who provided the mortgage money for these Italians? A cursory examination I made of Chicago's mortgage records indicates that there were Italian contractors, bankers, and steamship agents (for example, M. Bottigliero, Navigato Sons and Co., and G. Stefano) and other *padroni* types whose names appear frequently as creditors to fellow Italian homeowners. As much attention should be shown such people in a book on Italians as is given a non-Italian like John Powers.

Obviously, more research will have to be done on Chicago's Italians for the period under discussion. Some areas, such as the relationship of Italian-Americans to the arts, were hardly touched upon. And grass-roots leaders among the Italians will have to be ferreted out, their careers traced, and the history of Chicago's Italians integrated still further. I hope Professor Nelli will carry on. If not, he has made the way so much easier for the scholars who will certainly follow him.

LUCIANO J. IORIZZO
State University College,
Oswego, New York

SILVANO M. TOMASI and MADELINE H. ENGEL, editors. *The Italian Experience in the United States*. Staten Island, N. Y.: Center for Migration Studies. 1970. Pp. x, 239. \$8.00.

The Italian Experience in the United States is an excellent addition to the growing list of scholarly works in the area of Italian-American studies. The editors have selected ten articles on various aspects of the Italian experience in America. I shall comment only on those articles that offer re-evaluations of significant themes in Italian immigration history.

A major reappraisal of the controversial *padrone* issue is presented by Luciano J. Iorizzo. Effectively challenging the assumption that the *padrone* leadership retarded the assimilation of the Italian immigrants in America, Iorizzo depicts the *padroni* as providing vital economic opportunities that facilitated upward job mobility for their compatriots throughout the United States.

Equally convincing is Humbert S. Nelli's assertion that the "Little Italies" provided the

newcomers with transitional but essential institutions that accelerated their adjustment to American society. Nelli shows that the need for cooperation among the Italians led to the establishment of benefit societies, immigrant banks, and a colonial press. These stimulated the residential mobility that characterized Italian immigrant life during the early twentieth century. I reject, however, Nelli's argument that American social workers consistently supported the quest of the Italian immigrants for social justice. Beginning in 1908 social workers, experiencing increasing frustrations in their efforts to overcome urban problems, joined nativists who espoused immigration restriction as a panacea for urban ills.

Salvatore J. LaGumina provides an analysis of the importance of ethnic identity in politics, especially in the career of Vito Marcantonio. I share Silvano M. Tomasi's view that the ethnic Church contributed significantly in accelerating the integration of the Italians in America. Unacceptable, however, is Tomasi's conclusion that Italian socialists and anarchists were "so peripheral to the immigrant mass that they produced a handful of exceptional figures and nothing else." The activities of Carlo Tresca, Arturo Giovannitti, and other Italian radical leaders cannot be dismissed so lightly.

This book also contains interesting articles dealing with the contemporary Italian emigration to America, the public schools and the Italians, the religious acculturation of Italian-Americans, the return migration to Italy, and the contributions of Italians in the development of the labor movement in Argentina.

SALVATORE MONDELLO
Rochester Institute
of Technology

JOHN H. M. LASLETT. *Labor and the Left: A Study of Socialist and Radical Influences in the American Labor Movement, 1881-1924*. New York: Basic Books. 1970. Pp. vi, 326. \$10.00.

This study seeks to explain the failure of socialist, radical, and Left politics to capture the American trade-union movement during the Gompers era as they did in Europe. Unpersuaded by studies that focus largely on the ideology of national leaders in both realms, particularly on the antagonism between the AFL leadership and spokesmen for the Socialist

Labor and Socialist parties, John H. M. Laslett has undertaken a new approach. He identifies six labor organizations that illustrate the ethnic, structural, and geographical diversity of American trade unions and that at some time evidenced socialist or radical third-party tendencies: the brewery workers of the Midwest, the Irish shoe workers of New England, the ILGWU in New York City, the International Association of Machinists, the United Mine Workers, and the Western Federation of Miners. Laslett's narrative and interpretive essay on each case identifies the sources of socialist or radical strength as well as the reasons for its decline, although he encounters some difficulty in explaining complex causal relationships in essays of under fifty pages each. He skillfully combines the research of others with his own examination of the sources, and his case-by-case conclusions, or more properly his conjectures, are always tentative, modest, and suggestive.

In his concluding chapter Laslett generalizes from his individual studies and reviews several possible explanations for the failure of American socialists to create a permanent trade-union role for themselves comparable to that of their European counterparts. While affirming traditional explanations, ideological and otherwise, for the ultimate rejection of socialism and third-party activity by American trade unions, he would add indigenous American factors—social, economic, and political—derived primarily from our rapid industrialization.

Can Laslett's methodology fully answer a question so vital to our interpretation of socialism and the Left in this period? Even if one accepts the validity of his union sample one may question whether generalizations from this particular set of union-socialist relationships are adequate. Moreover, by concentrating on the experience of national unions, generally AFL affiliates, he does not entirely escape the myopia of previous scholars who emphasized ideological antagonism between national labor and socialist spokesmen.

On the other hand, the labor-socialist dalliances that Laslett cites were often local or regional, not truly national. Hence he might examine the influence of decentralization within the American labor movement and of

autonomy for local socialist parties, which together led to temporary or localized ties but seldom to across-the-board or continuing alliances. He might study union relations with socialists in those important urban areas where the socialists were strongest to discover what brought local alliances into being and what caused them to lapse.

Laslett's study ought also to suggest a re-examination of the classic European experience to determine whether American historians have properly interpreted the political relationship between labor and socialism there.

FREDERICK I. OLSON

University of Wisconsin—Milwaukee

CHARLES HOFFMANN. *The Depression of the Nineties: An Economic History*. (Contributions in Economics and Economic History, Number 2.) Westport, Conn.: Greenwood Publishing Corporation. 1970. Pp. lvi, 326. \$12.50.

Professor Hoffmann, an economist by training and by academic label and an obvious adherent of the "new economic history," analyzes the depression of the 1890s in the United States within a macroeconomic frame, using the Keynesian income-output flow analysis because he finds it particularly appropriate for dissecting an economy in the throes of cyclical crises. He believes—and he makes a very convincing case for it in his book—that such an approach to the problem of the nature and causes of the depression "appears to yield more productive results than the usual paths trod by the historian."

Hoffmann's opening chapter focuses on a forty-five-year period, 1869–1913, in order to highlight short-run business cycle fluctuations within the context of the secular (that is, long-run) economic trends and developments of a rapidly industrializing society. The emerging picture, reinforced by the growth traced in several macroeconomic indicators of economic activity, is that of a dynamic economy growing at relatively high rates (much higher than those of Western Europe) and of a pronounced secular decline in prices from 1869 to 1896, with the rates of decline reflecting changed proportions in national income and different rates of growth in the output of various products. The remaining seven chapters telescope the depres-

sion years, chapter 2 presenting a descriptive analysis of the most significant economic events of 1892–97 and subsequent sections concentrating on investment, consumption, the balance of payments, monetary policies and government fiscal operations, and prices.

In regard to the relationship, if any, between the "battle of the standards" and the depression, Hoffmann concludes that the depression resulted neither from an inflation precipitated by the Sherman Silver Purchase Act of 1890 (as was claimed by much of the financial and mercantile community) nor from there being too little money in circulation (as was charged by the Populists). Indeed, the money supply "grew proportional to growth in most parts of the economy" in the years preceding the panic of 1893 as well as during the ensuing depression; and the contraction of the money market just before the cyclical downturns of 1893 and 1896 was due to a complexity of economic factors in which currency policy played a contributing but not a dominant role.

There are over sixty tables, graphs, and series dealing with the economy of the 1890s, not to mention an excellent critical bibliography of statistical sources.

IRVING KATZ

Indiana University

NORMAN H. CLARK. *Mill Town: A Social History of Everett, Washington, from Its Earliest Beginnings on the Shores of Puget Sound to the Tragic and Infamous Event Known as the Everett Massacre*. Seattle: University of Washington Press. 1970. Pp. x, 267. \$7.95.

This is a beautifully written book that demonstrates once again the potential of local history, even when the sources are far from complete. Professor Clark, who teaches at the Community College in Everett, has succeeded in evoking the spirit of a turbulent past in his home town and the Pacific Northwest. Though the subtitle is quaintly rambling, the book tends to be a social history in the modern sense—concerned with class structure, life styles, social mobility, and conflict. Chapter titles such as "The Sawdust Baronage" and "The Iron Law" indicate the literary quality of the book; but the method is generally systematic, as Clark is, for example, in assessing responsibility for the "massacre"

of 1916. Together with recent studies of the IWW and the timber industry, this is indeed a useful volume.

Personalities are important to the account. "Jim" Hill's Great Northern Railway and John D. Rockefeller's investments were crucial in the early years of Everett. Other capitalists came and stayed, developing the land companies, mills, and banks, acquiring fortunes, and resisting with "fierce individualism" the activities of union men or others who tried to interfere. These capitalists and their allies, including deputies by the hundred in time of crisis, largely frustrated the efforts of moderate unionists or radicals. The competitive and sometimes chaotic timber industry gave partial justification to employers for their harsh labor policies. One gains a sense from the book of the drama of Everett's growth and its problems, of the varied leaders and their ideas, and of the social groups contending for control. Finally, almost inexorably, came the depression of 1913 with ensuing strikes, "free-speech" demonstrations, beatings administered to the Wobblies, and the bloody clash of 1916.

Perhaps the most striking statement in the book occurs in the epilogue as the author reflects on the "radical critique" and its validity, on the effects of technological change, and on the developing educational system. If, he says, "to be radical was to open new avenues of class mobility," then the most radical institutions of the time were the public schools, including high schools and junior colleges that did not charge tuition. Along with technological improvements in the mills, the schools quietly brought new opportunities to the "wage slaves" of Everett.

J. LEONARD BATES
University of Illinois,
Urbana-Champaign

HAROLD T. PINKETT. *Gifford Pinchot: Private and Public Forester*. Urbana: University of Illinois Press. 1970. Pp. 167. \$6.95.

Gifford Pinchot's career as an active forester began in 1892 when he was hired by George W. Vanderbilt to plan the management of Biltmore Forest in North Carolina. It ended in 1922 when he resigned as Pennsylvania commissioner of forestry in order to run for governor. His

most influential years were from 1898 to 1910, during which he developed the U.S. Forest Service and became a symbol of the conservation movement. He was so incredibly innovative and productive that Harold T. Pinkett is at times hard pressed in the space he allows himself in this succinct monograph to catalog Pinchot's achievements and contributions.

Pinkett's careful chronicle, which won an award from the Agricultural History Society in 1968, indicates that Pinchot the forester has perhaps been given too little credit while Pinchot the conservationist has sometimes been given too much. Although Pinchot talked and wrote about the need to conserve all natural resources, he reserved his highest enthusiasm for lumber-producing forests. He enlarged his arguments to include water, soil, and minerals, but he showed little warmth for the crusade for wild-life and no sympathy for the incipient wilderness movement.

Pinkett's admirably terse and disciplined study, illustrating how much a skillful writer can say on a large subject in a small space, is one of three recent scholarly monographs on Pinchot. It is the only one to concentrate on details of Pinchot's work as a forester. Nelson M. McGeary's *Gifford Pinchot, Forester-Politician* (1960) treats forestry largely as a part of a political career. Martin L. Fausold's *Gifford Pinchot, Bull Moose Progressive* (1961) examines Pinchot's role as a political leader from 1910 to 1917. These works do not exhaust the subject of Gifford Pinchot. He merits further monographs and ultimately a full-scale narrative biography. The source material has colossal proportions. The Gifford Pinchot Papers at the Library of Congress are twice as voluminous as the combined papers for the three presidents under whom he served: McKinley, Roosevelt, and Taft.

OLIVER H. ORR, JR.
Washington, D. C.

MORRELL HEALD. *The Social Responsibilities of Business: Company and Community, 1900-1960*. Cleveland: Press of Case Western Reserve University. 1970. Pp. xix, 339. \$9.95.

Professor Heald has built upon his earlier articles to produce a detailed treatment "of the ideas and activities through which American

businessmen, in the course of the first sixty years of the twentieth century, have attempted to define and respond to the relationship of their firms to the surrounding community." Given this definition, the reader is puzzled by the inclusion of the word "company" in the subtitle, for the narrative only incidentally reflects the social gospel of business in terms of labor relations or similar internal affairs.

The late nineteenth century saw little movement in the direction of business statesmanship; however, with the Progressive era, "thoughtful businessmen" were becoming sensitive to the adverse effects of urbanization and economic consolidation. During the succeeding two decades, as business organization and control were undergoing change, the ethic of social responsibility was explored by the managers of big business. In involving their companies in community chest campaigns and later (by the 1950s) in educational and cultural programs, the managers illustrated bureaucratic genius but little substantive contribution in terms of relative dollars and cents. Corporate philanthropy amounted to an average of one per cent of net profits in the years from 1936 to 1960 (even though a total of five per cent was allowable for tax purposes). What is more, small enterprises appeared to contribute proportionally more than the large corporations.

Throughout the book, Heald is too tolerant of the business record and at times is weighing straws of evidence. For example, he balances the ledger of the one per cent corporate tithe with the statement that "loans of company personnel, gifts in kind and services, use of company clerical and other facilities, payroll deduction plans, sponsorship of community activities, advertising, and a host of similar services added untold amounts to the values actually contributed by business firms. The cost of such gifts of time and talent was incalculable—an accountant's nightmare."

Moreover, Heald is too uncritical of the credo books of big businessmen in which he claims to have found evidence for a broader concept of social responsibility. Some of these works are pure puff, unsubstantiated by actions. The book is further marred by awkward sentences and plural subjects tied to singular verbs. But the main defect is its anachronistic quality; ten years ago, when our social expectations were

lower and our social consciences less critical of token gestures, *The Social Responsibilities of Business* would have been part of the times; however, against the background of Ralph Nader, the ecology crisis, oil depletion allowances, and automobile workers vengefully stuffing Coke bottles into new car bodies, Heald's evidence pales.

THOMAS V. DIBACCO
American University

ARTHUR A. GOREN. *New York Jews and the Quest for Community: The Kehillah Experiment, 1908-1922*. New York: Columbia University Press. 1970. Pp. x, 361. \$10.00.

In the issue of the *North American Review* for September 1905 there appeared an article written by the police commissioner of New York City entitled "Foreign Criminals of New York," which began: "It is not astonishing that with a million Hebrews, mostly Russians, in the city (one quarter of the population) perhaps half of the criminals should be of this race. . . . Among the most expert of all the street thieves are Hebrew boys under sixteen, who are brought up to lives of crime. . . . The juvenile Hebrew emulates the adult in the matter of crime percentage. . . ."

This blunt accusation shocked the Jews of New York City into establishing perhaps the most ambitious and comprehensive religio-ethnic community structure in the American experience. Their horrified rejection of what was interpreted as a gratuitous anti-Semitic slur and the equally horrifying possibility that the article was less anti-Semitic than it was revealing, were two powerful incentives to organize an American model of the traditional European self-contained Jewish community, called the Kehillah.

Although the New York Kehillah began as a response to a crisis, it soon began to respond to a larger need—to create a Jewish organizational structure and atmosphere that could sustain Jewish life and yet be fully in harmony with the American environment. The account of this fascinating fifteen-year experiment in subculture community organization is described in the well-researched, fully documented, and thoughtfully interpreted book by Arthur Goren. In Goren's volume we recognize that the founders and moving spirits behind

the Kehillah (an amalgam, representing both the acculturated German Jews and the East Side's largely Yiddish-speaking Eastern Europeans) were not attempting to insulate the Jewish community behind a ghetto façade. They were, on the contrary, embarked on a bold and ambitious attempt to vindicate their understanding of the American creed by erecting cultural dams and bridges that might both generate Jewish values and distribute them into American society.

The experiment failed and the Kehillah disappeared. Perhaps it could not be successfully transplanted from its European environment, although it was partially reborn in the Jewish Community Councils that dot the American landscape today.

There is, however, a possibility that the Kehillah was an idea whose time was premature. Today the resurgence of ethnic and racial pride with renewed interest in the value and possibilities of cultural continuity "beyond the melting pot" suggests a fresh and useful look at the Kehillah experience.

Two questions were asked by the Jews of New York in 1908. Is a continuing religio-ethnic subculture in America considered to be a positive good? If so, how much and what kind of internal organization and inner concentration are necessary to assure its effective and creative existence? The answers given then in terms of integration and separation can be helpful to those who today are balancing the same twin needs and experimenting with similar responses.

MAX VORSPAN
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P. EDWARD HALEY. *Revolution and Intervention: The Diplomacy of Taft and Wilson with Mexico, 1910-1917*. Cambridge, Mass.: MIT Press. 1970. Pp. 294. \$10.00.

HERBERT MOLLOY MASON, JR. *The Great Pursuit*. New York: Random House. 1970. Pp. 269. \$8.95.

Professor Haley has set himself the task of describing the American reaction to the turbulence in Mexico from 1910 to 1917. By quoting at length from the reports of American representatives in Mexico and from the suggestions and policy statements that came out of Washington, he presents a detailed and descriptive

account of all the events, major and minor, that affected Mexican-American relations.

Although President Taft gave some thought to the possibility of intervening, he never really considered it seriously, in part because he thought such action required prior congressional approval, but largely because he believed it would never solve the problem. Despite the reluctance of Congress to approve, President Wilson ordered troops into Veracruz. Haley feels that after the confrontation with President Huerta had ended, Wilson turned his attention to Europe and that his subsequent preoccupation with German-American relations gave Mexico a respite in which to draw up and promulgate the revolutionary constitution of 1917.

Haley's title contains the word "revolution," and his objective is to describe the American reaction to revolution. The difficulty is that he assumes a situation that did not exist. Civil war and violence, yes, but not revolution. The economic and social revolutionary forces did not jell until after the constitution of 1917 went into effect.

Although Haley's research in State Department material is thorough, he has overlooked a number of recent American publications, such as Krieb's study of the United States and Huerta and Parrini's and Levin's studies of Wilson's overall policy, and he has completely ignored Mexican authorities. In short Haley has covered the same ground as Link, Cline, Coletta, and Quirk and has added some new details.

Mason's account of Pershing's expedition is a good journeyman's report with none of the scholarly apparatus. Beginning his tale with Villa's raid on Columbus, New Mexico, he then gives a day-by-day chronicle of the Pershing expedition into Mexico and the Wilson-Carranza negotiations, which finally led to the withdrawal of the American troops.

WALTER V. SCHOLÉS
University of Missouri,
Columbia

B. R. BRUNSON. *The Texas Land and Development Company: A Panhandle Promotion, 1912-1956*. (The M. K. Brown Range Life Series, Number 9.) Austin: University of Texas Press. 1970. Pp. xi, 248. \$7.50.

The Llano Estacado, a distinctive landform of the Great Plains of the United States, lies in western Texas astride its border with New Mexico. Until the last quarter of the nineteenth century, Anglo-Americans considered this high, flat tableland with its sparse rainfall and vegetative cover of short buffalo and grama grasses generally devoid of economic and settlement potential. After the subjugation and removal of the Plains Indians, however, this vast area was rapidly carved up into large cattle ranches. Most of these great spreads were short-lived and were soon broken up into small parcels to be sold to individual farmers by land companies that flourished in northwestern Texas in the late nineteenth and early twentieth centuries.

B. R. Brunson has carefully reconstructed the history of one of these organizations, the Texas Land and Development Company, from information gleaned mainly from company records, from personal interviews and correspondence with former company employees and officials, and from such other sources as public records and contemporary newspaper files. The study is well written and well documented, and it includes a thorough description of the business and financial complexities involved in the formation and functioning of this distinctive enterprise.

The Texas Land and Development Company began its operations in 1912 in the Plainview area of the Llano Estacado, near the southern edge of the Texas Panhandle. Although somewhat similar to other land-promotion schemes in the region, this company also had a number of unique features, including the fact that it purchased relatively high-priced land, totaling 61,360 acres, in an area that had already been settled. Moreover, the company attempted to sell this property in the form of fully developed farms, complete with buildings, fences, plowed fields, orchards, and irrigation systems with deep wells that tapped the great aquifer underlying the Llano Estacado.

This ambitious venture, which lasted until 1956, resulted in an overall financial loss, and most of the property was eventually sold in an unimproved or only partially improved state. The Texas Land and Development Company made a substantial contribution to the economic well-being and growth of the Plainview area, however, especially to its agricultural de-

velopment through crop experimentation and the promotion of irrigation agriculture.

WILLIAM B. CONROY

Texas Tech University

FRANK L. GRUBBS, JR. *The Struggle for Labor Loyalty: Gompers, the A. F. of L., and the Pacifists, 1917-1920*. Durham: Duke University Press. 1968. Pp. viii, 172. \$7.50.

This slender yet well-researched book reinforces the image of Samuel Gompers and the American Federation of Labor as willing and even obsequious servants of the foreign policy of Woodrow Wilson during World War I. Grubbs's findings substantiate the broader interpretations of American labor and Wilsonian diplomacy broached by Arno Mayer in his *Politics and Diplomacy of Peacemaking* (1967) and Ronald Radosh's *American Labor and United States Foreign Policy* (1970). But where Mayer and Radosh focus more on the overseas activities of American labor leaders, Grubbs stresses Gompers' role in combatting domestic opposition to the Wilson administration by the People's Council of America, an antiwar coalition of pacifists, social reformers, radicals, and revolutionaries. As this book clearly proves, the patriotic crusade of the American Alliance for Labor and Democracy, Gompers' prowar alternative to the People's Council, was financed and supervised in Washington by George Creel's Committee on Public Information.

Grubbs's method of interpretation and analysis, however, leaves much to be desired. His treatment of the People's Council is comparable to Lyndon B. Johnson's and J. Edgar Hoover's response to protest against the war in Vietnam. The People's Council peace program, which was in fact shared by antiwar factions in all belligerent nations, is labeled by Grubbs as the Bolshevik plan for peace. Again and again Grubbs associates opposition to the Wilson administration with Bolshevism. And he compounds this guilt by association with a myriad of small errors and also some quite major historical distortions. For example, Grubbs refers to Kerensky's government as socialist Russia, exaggerates Bolshevik influence in Russia and elsewhere prior to November 1917, and implies that the Socialist party of America actively sought to obstruct the American war effort. The most glaring minor error credits the A. F.

of L. with a membership of thirteen million in 1919, when the actual figure was closer to five million. Minor errors, though of little significance in themselves, indicate Grubbs's careless approach to a complex historical problem. Crammed with vital information on the domestic opposition to World War I, this book must nevertheless be read with caution.

MELVYN DUBOFSKY
University of Wisconsin—
Milwaukee

BERNARD K. JOHN POLL. *Pacifist's Progress: Norman Thomas and the Decline of American Socialism*. Chicago: Quadrangle Books. 1970. Pp. xiii, 336. \$8.95.

Pacifist's Progress is not a biography of Norman Thomas, two of which already have been published, but an inquiry into Thomas' responsibility for the extinction of the Socialist party of America. That question is part of a larger question as to why the United States is the only major country in which socialists have failed to mount and sustain a major movement. Professor Johnpoll's carefully researched but unflaggingly readable book is of interest, therefore, to historians not just of America but of other countries as well.

Thomas himself liked to say that his party went out of business because the New Deal stole its immediate demands and put them into the statute books. Professor Johnpoll grants that there is some truth to the assertion and adds that other nonsocialist reform movements, such as the Fusion movement behind La Guardia, also splintered the Socialist party and syphoned off votes that might have gone to Thomas and his comrades. Socialism fell before liberalism.

But there is a deeper explanation still, according to Professor Johnpoll—namely, Thomas' "inability to conceive of politics as the art of choosing among possible alternatives." From the time he joined the SPA in 1918 as a Christian pacifist until his death fifty years later, Thomas was an "ideologue" who did not know how to get things done. His vanity and contentiousness, Professor Johnpoll observes, made a badly divided party more divided still. Nor did Thomas know, quite, how the world worked. Not until the late 1930s did he perceive the totalitarianism in Soviet communism. Only after

Pearl Harbor did he recognize the threat of the Fascist axis.

All this and much more Professor Johnpoll documents in convincing detail. He has written an important book on how not to succeed in American politics. But he overstates his case, I think, in claiming that Thomas in 1928 took over a healthy party. The Socialists had been mortally wounded a decade earlier by World War I and the Bolshevik Revolution. Norman Thomas did not kill the SPA—he presided over a delayed funeral.

ARTHUR MANN
University of Chicago

THOMAS H. BUCKLEY. *The United States and the Washington Conference, 1921-1922*. Knoxville: University of Tennessee Press. 1970. Pp. ix, 222. \$8.50.

As its fiftieth anniversary approaches the Washington Conference appears headed toward an improved reputation. Such a fluctuation in reputation depends in part, of course, on the fluctuating concerns of the commentators. D. F. Fleming and other writers of the 1940s and 1950s found the conference notable, in an adverse sense, because it had brought the "illusion" of peace and thereby deflected Americans from the duty of perfecting a "realistic" collective security combination. Today, in a time of negotiations aimed at strategic arms limitation, the conference, according to the book under review, "deserves remembrance because it was the only successful arms limitation conference in modern history." Fortunately, however, Professor Buckley spends only a few lines in the pursuit of relevancy, and his conclusion—that peace is impossible through mere disarmament unaccompanied by political settlement—is as unobtrusive as it is unsurprising.

The bulk of this book deals, rather, with the issues and events of the conference itself, set in the context of 1921-22. Thus examined, the Four-, Five-, and Nine-Power treaties and the other results of the conference appear as largely satisfactory accommodations to the interests of American diplomacy. Only in the agreement on Pacific fortifications and in the ambiguous disposition of the China problem does the author find the American bargainers to have made noteworthy mistakes. Otherwise he agrees with Senator Lodge's contemporan-

eous (and self-interested) judgment that the conference was "on the whole pretty well done."

The uncomplicated narrative contains no bombshell revelations, but it is studded with the kind of factual data that gives body to historical reconstruction. Two examples will suggest what the Japanese and British archives have yielded: Elihu Root, an American delegate, more than once secretly gave to the Japanese information that a person in his position should not have; Winston Churchill persuaded the British cabinet to adopt for possible use against the Americans the bargaining device used earlier against the Germans, a paper program of naval construction. Such items, interesting individually, become more so as they join others to form the most satisfying explanation yet of the way in which the American, British, and Japanese positions converged in 1922. Other studies now under way will surely supplement this one, but they are unlikely to displace it.

W. B. FOWLER

University of Washington

DONALD L. WINTERS. *Henry Cantwell Wallace as Secretary of Agriculture, 1921-1924*. Urbana: University of Illinois Press. 1970. Pp. x, 313. \$8.95.

This volume should go a long way in filling a gap that historians interested in the study of agricultural distress in the early 1920s have recognized as needing to be filled. The author, a young Ph.D., based his findings on the files of the secretary of agriculture, the Bureau of Agricultural Economics, the secretary of commerce, the Bureau of Foreign and Domestic Commerce, and the secretary of the interior—located mostly in the National Archives—explored the most pertinent available primary sources, and has lent substance to what the better informed agricultural historians have long suspected. This, in itself, is a notable contribution. Unfortunately, the very materials that furnish strength to the study are also the very materials that reflect on the study and on Henry C. Wallace who was hardly the exciting, inspiring, dramatic personality that his son became a decade later. Wallace was an honest, diligent, hard-working, but dour man who in some respects was out of place in the cabinet of the sporty Warren G. Harding. It is difficult

to imagine two more different men depending on each other for advice and encouragement in the relief of the American farmer.

Winter has traversed some familiar territory, but this was unavoidable in furnishing the background for the man who during his brief span as secretary of agriculture was at the nerve center of the farmer's quest for justice. The author had the good sense to minimize the importance of the much publicized Agricultural Conference of 1922, which, for all practical purposes, was a complete dud. His treatment of the difficulties and rivalries between Wallace and Herbert Hoover is the fullest in print and probably the most significant contribution of the book. Even though the volume gives strong evidence of a scholar who has entered the field with a strong orientation in political and intellectual instead of social and economic history, it still comprises a remarkable addition to scholarship in an area that often has been ignored by historians of this persuasion. Students of the 1920s and agricultural historians in general will find this a welcome addition to their reading lists and personal libraries. A brief but helpful bibliographical essay and a workable index add to the value of the study.

THEODORE SALOUTOS

*University of California,
Los Angeles*

CHARLES MCKINLEY and ROBERT W. FRASE. *Launching Social Security: A Capture-and-Record Account, 1935-1937*. Madison: University of Wisconsin Press. 1970. Pp. xxiv, 519. \$12.95.

In recent years historians have devoted increasing attention to the study of large-scale organizations and their impact on American society. An "organizational synthesis" of American history has even been suggested. Yet modern bureaucracies pose new and difficult problems for the researcher. They generate masses of documents, make decisions by committee, and often consign the implementation of policy to people who had no role in making it. To fight their way through the resulting tangle, historians have resorted to various expedients, including team research and oral history. Now it appears that two social scientists, working as historians, anticipated these problems and their possible solutions more than thirty years ago.

Between November 1935 and May 1937 Charles McKinley and Robert Frase attempted to "capture and record" the internal history of the new Social Security Administration. Although their background and interest were in public administration, their approach was largely historical. By reviewing the available documents and, more important, by talking to the men and women involved, they were able to piece together a coherent account of the organization's growth and operation in its crucial early months. But, like more recent historians who have studied private or public bureaucracies, they found their job increasingly difficult as the organization grew. Finally, Frase tells us, he resorted to a "shotgun attempt to cover everything" (p. xxiv). The manuscript they compiled between 1937 and 1941 includes long chapters on the work of the Social Security Board and the major bureaus, as well as on budget making, the structure of the organization, and "general management."

The results of this effort are intriguing and can support either a liberal or radical interpretation of federal government activity and the social security system. McKinley and Frase show that the Social Security Board set high standards, especially for the recruitment of personnel. Indeed, the zeal and political sensitivity of the board members led to a refusal to delegate authority and frequent interference in routine administrative matters. On the other hand the energies of the staff were often absorbed in bureaucratic in-fighting and factional conflict. These disputes, however, involved personal preferences or ambitions rather than policy matters. When the Republicans attacked the Social Security Act in the 1936 elections, administrators at all levels joined in the effort to counter the partisan charges and to re-elect the Democrats. Despite these problems, the agency—if not the system—seems to have operated efficiently. The board initiated the old-age pension system without major difficulty but had more trouble with federal-state unemployment compensation and was able to make only modest progress in reforming the state public assistance programs. The contrast between the high quality of the organization and the often poor quality of the results, especially in the "welfare" area, is striking.

Because of their closeness to the subject, Mc-

Kinley and Frase had considerable difficulty in deciding what to include or omit. In general they seem to have solved their problem by including a little of everything. This strategy was consistent with the "shotgun" approach, but it will discourage general readers. For specialists, however, *Launching Social Security* is a major addition to the growing historical literature on organizations and the American system of social insurance.

DANIEL NELSON

University of Akron

The Wartime Journals of Charles A. Lindbergh. New York: Harcourt Brace Jovanovich. 1970. Pp. xx, 1038. \$12.95.

Colonel Lindbergh's journal entries run from March 1938 to June 1945 with the exception of two notable gaps. The journals are printed unedited except for some justifiable omissions, and their usefulness is enhanced by accurate identifications in footnotes located at the bottom of the page.

These annals will prove invaluable to Lindbergh's future biographers and students of aviation history. Lindbergh mirrors the ethic of small-town America, displaying the familiar syndrome of prejudices against both non-Nordic stocks and the eastern seaboard Establishment. The loss of his privacy at the hands of news-hungry reporters, climaxed by the tragedy of 1932, alienated him from FDR's America. His ardent desire to escape recognition in public places and to live without prying eyes with his adored wife and later-born children bordered on the pathological. In a revealing passage he notes that he moved about with maximum freedom in Nazi Germany. Lindbergh presciently sensed some of the ills of contemporary America, yet this book will bring small comfort to present leftist critics since he champions military training, approves of imperialism, and opts for fascism rather than communism as the choice of the lesser evil for Europe and Asia.

Much of what Lindbergh records deals with technical aviation matters in Germany, Britain, and the United States where during the war he served as a trouble shooter for Ford and other airplane manufacturers. In this capacity he visited the South Pacific front in 1944 to observe aircraft under combat conditions. He

draws a finely etched portrait of the war in these parts, and offers additional confirmation of both American and Japanese barbarities. Lindbergh proved inflexible even at forty, for he found it difficult to become acclimated to multiengine planes operating under ground control. Essentially a natural scientist, his descriptions of the air, sea, and terrain are moving. In the aeromedical field Lindbergh used himself as a guinea pig to measure the effects of various types of airmanship on the human body.

Lindbergh's lengthy account of his part in the prewar isolationist front will necessitate little revision of our present historical judgment. He was purblind to the Nazi danger until war broke out and thereafter blamed the West for not averting it. A pioneer Pearl Harbor revisionist, he tried to persuade Henry Ford to subsidize Harry Elmer Barnes in writing a second round of war-guilt revelations. Lindbergh relays some useful information on the origins of America First, the tensions within the organization, and the activities of local chapters who sponsored his anti-interventionist speeches.

A keen observer of scientific and natural phenomena, Lindbergh was a poor guesser in the field of international politics. He grossly underestimated British ingenuity and stamina and mistook for apathy America's grim determination to win a knockout victory that he thought impossible as late as 1942. On the morrow of the Nazi surrender Lindbergh went to Germany to study wartime enemy advances in aviation and rocketry. He was competent here as in other missions, but he once more displayed his insensitiveness to the chemistry of human behavior by equating the planned Nazi genocide with the battlefield conduct of American soldiers on the Japanese front.

SELIG ADLER

*State University of New York,
Buffalo*

RICHARD R. LINGEMAN. *Don't You Know There's a War On? The American Home Front, 1941-1945*. New York: G. P. Putnam's Sons. 1970. Pp. 400. \$7.95.

Richard R. Lingeman, a member of the staff of the *New York Times Book Review*, has written a breezy, popular account of life in the

United States during World War II. His book has a number of virtues. It is clearly written, straightforward, and never pedantic. It discusses aspects of popular culture—motion pictures, sports, popular music—that historians have too often slighted. The author devotes considerable attention to wartime migration, civilian defense, and the black market, but his longest chapters are entitled "Will This Picture Help Win the War?" and "Pleasures, Pastimes, Fads and Follies."

Yet if the book has strengths it also has defects. Lingeman apparently cannot pass up a pun (because shortages prevented the use of rubber in girdles, he remarks, "a variety of tactical plans was worked out to fight the home front's battle of the bulge"). His judgments about political and economic developments are sometimes shaky and his facts wrong. Charles E. Wilson of the War Production Board was not "formerly of General Motors" but formerly of General Electric; Henry Wallace headed the Board not the Bureau of Economic Warfare; the Office of Emergency Management was not absorbed into the Office of War Information (although its Division of Information was); Thurman Arnold never served as attorney general. Then too, for a book that places such heavy emphasis on social trends and, indeed, on Willow Run as the archetypal boom town, there are surprising bibliographical omissions; there is no mention, for example, of Francis E. Merrill, *Social Problems on the Home Front* (1948), Robert J. Havighurst, *Social History of a War-Boom Community* (1951), or Lowell J. Carr and James E. Stermer, *Willow Run* (1952).

At times Lingeman successfully conveys a sense of the quality of life in wartime America, but too often he fails to offer any analysis or evaluation and simply strings anecdotes together in an attempt to awaken feelings of nostalgia. The book then becomes a source for answering such questions as who played for the 1944 American League champion St. Louis Browns, or how Clark Kent got a 4-F deferment. As the publisher's blurb says, "for those over thirty who were in this country then, the book will be a trip down Memory Lane. For others it will be pure history." Unfortunately, those who venture down memory lane are apt to become lost in a forest of trivia. General readers

may find the trip pleasant enough, but few historians will find it instructive.

RICHARD POLENBERG
Cornell University

ATHAN G. THEOHARIS. *The Yalta Myths: An Issue in U.S. Politics, 1945-1955*. Columbia: University of Missouri Press. 1970. Pp. viii, 263. \$10.00.

This is a straightforward account of the criticisms leveled at Yalta in the decade following that conference. It is based largely on published material, including the *Congressional Record*, Senate and House hearings, political campaign literature, newspapers, and magazines. These are supplemented by interviews and a few items from the Truman Papers.

Theoharis does not examine critically the conference and the charges against it. He labels the criticism irresponsible and proceeds from there. He lists four principal myths: that Alger Hiss was an important architect of the Yalta agreements, that Roosevelt's mental acuity was failing at the time of the conference, that Yalta brought about the communization of Eastern Europe, and that Yalta caused the fall of China to the Communists. Except possibly for the second item, the author is probably justified in assuming that these have already been proved to be myths. In order to bring some order out of the chaos of charges Theoharis divides the critics into three categories. The bitterest critics he labels "extremists." They included such luminaries as Westbrook Pegler and Senators Joseph McCarthy, Robert Taft, William Jenner, and Everett Dirksen. The mildest group is designated "moderates" and included, among others, John Foster Dulles, Thomas Dewey, and Senators Arthur H. Vandenberg and Henry Cabot Lodge. Occupying a middle ground between the "moderates" and "extremists" were the "partisans," who included critics such as Senators Richard Nixon and William Knowland and Representative Walter Judd.

In the period from 1945 to 1949 only the extremists vigorously criticized Yalta, but in 1949 the perjury trial of Alger Hiss and the fall of the Chiang Kai-shek regime led the partisans and moderates to seize upon Yalta for political advantage. Once in power in

1953, however, the moderates were embarrassed by the issue, and Eisenhower reneged on his campaign promise to repudiate the Yalta agreements.

It is an important story, and this recounting will contribute much to an understanding of the relationship between partisan politics and foreign policy. Future studies will doubtless reveal more of the inside story as scholars delve into the unpublished papers of the period.

RAYMOND A. ESTHUS
Tulane University

R. HARRISON WAGNER. *United States Policy toward Latin America: A Study in Domestic and International Politics*. Stanford: Stanford University Press. 1970. Pp. 246. \$7.95.

In this perceptive analysis of postwar Latin American policy, Professor Wagner stresses two constant themes of the United States: "It has judged the relevance of any proposed form of inter-American cooperation in the light of its central concern, namely, preventing any event that might invite a challenge to United States military power in the Western Hemisphere. And it has attempted to accomplish this objective with the smallest possible drain on its political and economic resources." In delineating these themes, Wagner joins that school of thought stressing military security as the most important objective of United States policy; sometimes business interests profit, but incidentally. He is not overly critical because there were only modest benefits for Latin America; indeed, he argues that American economic policies "have been reasonable attempts to serve a much more complex set of purposes than the purposes implied in most critical discussions."

Professor Wagner, a political scientist, uses the 1945-61 period to illustrate his thesis that a combination of complex international and domestic forces has determined United States policy. Within this context, Wagner seems to conclude that little more could have been done. When the United States did move ahead, with such things as the creation of the International Finance Corporation (1954), the Inter-American Development Bank (1958), and the Alliance for Progress (1961), it was only because of crises

that were initiated by international communism and that brought about renewed United States concern for its military security.

Readers looking for a historical treatment of postwar economic policy will find it here only in modest outline. What they will find in more detail is a well-done analysis of the domestic and international forces affecting the decisions of the Truman and Eisenhower administrations on economic policy for Latin America.

The documentation—over four hundred notes for two hundred pages—is impressive. A formal bibliography would have been helpful. This is a useful book that will be of interest to economists and political scientists as well as historians.

ROGER R. TRASK
Macalester College

ERIK BARNOUW. *The Image Empire*. (A History of Broadcasting in the United States. Volume 3—from 1953.) New York: Oxford University Press. 1970. Pp. 396. \$9.75.

The final volume of a three-part history of American broadcasting, *The Image Empire* begins with an illuminating analysis of television programming in the early 1950s. But as the story approaches 1970, it turns increasingly from description to dogma. In the end its central thesis is that the broadcasting industry is essentially an instrument for extending business-militarist imperialism abroad while tranquilizing and distracting domestic opinion.

This thesis may well bear further investigation. In this book, however, the argument does not seem convincing—in part, perhaps, because it is presented with so few reservations and in part because the documentation for the charge is highly selective and circumstantial.

The book's many virtues include a fascinating discussion of early television drama and excellent accounts of developments such as the television documentary, the telefilm, and public television. Considerable attention is also given to the Voice of America, Radio Free Europe, and various commercial evidences of America's "image empire" abroad. The entire study is extremely well written, though its strict chronological framework frequently requires the author to jump abruptly from one subject to another.

Many topics receive surprisingly little attention in this work, among them the rise of the "talk shows," variety and musical programming, children's programs, and sports coverage. With the exception of Edward R. Murrow no broadcasting figure is treated at any length; all of the important characters are political leaders. Even in the political realm major subjects such as campaign advertising and the disputes over the equal time and fairness doctrines are only touched upon. The relationship between television and domestic unrest, including the observations of the Kerner Commission, is also overlooked.

One book cannot do everything. But the omissions of this book lead one to wish that somewhat less space had been given to familiar political narratives, many of which are merely rewritten from other secondary accounts. Occasionally political material is carelessly presented. It is difficult, for example, to see how the author can confuse the opportunistic motives of Joseph McCarthy with the rigid moralistic motives of Secretary Dulles, no matter what judgment he passes on their actions. The author not only misquotes President Johnson at one point, but also identifies the line as the conclusion of a 1964 speech when actually it came in the middle of a speech given in 1965.

The Image Empire is always highly interesting, but it is at its best when it focuses on the internal dynamics of the broadcasting industry. When it moves in other directions, it becomes less scholarly, less original, and less persuasive. Yet the author's major concerns seem to lie precisely in these other directions as is demonstrated by the fact that the single document included in the appendix to this study is the CIA Act of 1949.

LEE W. HUEBNER
The White House

ROBERT G. WEISBORD and ARTHUR STEIN. *Bittersweet Encounter: The Afro-American and the American Jew*. Foreword by C. ERIC LINCOLN. (Contributions in Afro-American and African Studies, Number 5.) Westport, Conn.: Negro University Press. 1970. Pp. xxvii, 242. \$11.50.

Professors Weisbord and Stein have made the first attempt to survey the course of black-Jewish relations in American history. They have chosen to give more attention to Negro feelings

about Jews than to Jewish attitudes and actions toward Negroes; to concentrate on recent developments; and to focus on events in New York. For sources they have depended chiefly on existing monographs, newspapers, reports of private and public agencies, and polemical and analytical articles in the Jewish and Afro-American magazines. They do not utilize manuscript collections, organizational archives, or interview materials.

The book simply scratches the surface. New York is not the nation, and for valid generalizations we need analyses of Negro-Jewish relations in other cities. Clearly concerned with exploring and commenting on the tensions arising from race prejudice among Jews and from anti-Semitism among Negroes, the authors have frankly stressed the bitter side of the "Bittersweet Encounter." They give detailed attention to such matters as the Ocean Hill-Brownsville school dispute and the controversy that arose over the anti-Semitic sentiments expressed by a leader in the Mt. Vernon, New York, CORE chapter. The book is thus unbalanced, and inadequate attention is accorded to the disproportionate support given by Jews to the Negroes' cause over the years—a fact well recognized by blacks as the public opinion polls demonstrate. Thus, for example, while the authors refer to the fact that Jews have played a particularly important role in the twentieth-century civil rights movement, specific data are lacking. Important figures like Joel and Arthur Spingarn, Jack Greenberg, and Morris Milgram are referred to quite casually. A number of key individuals are simply not mentioned—including Louis Marshall, counsel for many critical NAACP court cases, especially in the fight against the white primary; Stephen Currier, who heavily subsidized the Southern Voter Education Project in 1962–64; and other leaders and financial pillars of the movement. Again in the case of CORE, the role of chief counsel Carl Rachlin and of Marvin Rich, the organization's influential community relations director, has not been examined. In another area the authors fail to deal with the contributions that Jewish scholars have made to black studies—from Franz Boas, Melville Herskovits, and Otto Klineberg, through Frank Tannenbaum and Herbert Aptheker, to Leon Litwack, Gilbert Osofsky, and Gary T. Marx.

What the authors have demonstrated very successfully is that in the encounter between blacks and Jews lies an important area for future research and investigation.

AUGUST MEIER

Kent State University

JOHN GALLOWAY. *The Gulf of Tonkin Resolution*. Rutherford, N. J.: Fairleigh Dickinson University Press. 1970. Pp. 578. \$18.00.

HENRY F. GRAFF. *The Tuesday Cabinet: Deliberation and Decision on Peace and War under Lyndon B. Johnson*. Englewood Cliffs, N. J.: Prentice-Hall. 1970. Pp. 200. \$6.95.

TOWNSEND HOOPE. *The Limits of Intervention (An Inside Account of How the Johnson Policy of Escalation in Vietnam Was Reversed)*. New York: David McKay Company. 1969. Pp. ix, 245. \$5.95.

Little more than one-fourth of John Galloway's absurdly expensive book consists of historical narrative; the remainder is made up of fifteen appendixes, most of which are available elsewhere in government documents. The author's account adds very little to what we already know about the Tonkin Gulf resolution. His description of the atmosphere in the Senate chamber on the day Senator Fulbright assured his colleagues that "our boats did not convoy or support or back up any South Vietnamese naval vessels that were engaged in . . . attacks" is probably the best section in this treatment of those fateful weeks in 1964 when an uninterested Congress thought it was giving the president a "*pro forma* expression of American patriotism."

This last insight, Lyndon Johnson's analysis of the likely North Vietnamese reaction to the resolution, was told to Henry Graff, but it accurately describes the atmosphere on Capitol Hill where, as Galloway says, the short debate was sandwiched between sundry topics, including the Big Horn National Recreation Area. Graff's book is an inquiry into the pathology of the Vietnam War and the progressive degeneration of the decision-making process at the highest level. Not that the author had intended it to come out that way when he began his conversations with the president and his aides in 1965. Far from it. His aim, and that of those who had invited him to Washington, was to present a sympathetic account of the

goings-on inside the president's closest circle, those who met with him for Tuesday lunch inside the family dining room of the White House. No one talked about where or when Graff's report would appear, but the White House Press Office knew that he had a first refusal agreement with the *New York Times Magazine*.

Graff began the first of his several visits to Washington when the hawks were all in full plumage and bustling about brimming with confidence in themselves and their mission; the last time he saw President Johnson he concluded that the latter believed "he had been led down a slippery path by men he had relied on too implicitly, and that he would willingly barter anything he owned or deserved for the chance of being able to retrace his steps." Graff prepared three articles for the *Times Magazine*, two of which appeared there. The third, notes Graff, did not seem to have the immediacy of news, and has not been printed before. Graff proved to be a genius in selecting his questions so that his interviewees could reflect upon such matters as the "Munich Analogy" (Johnson pushed it back once to 1917—"the Kaiser thought we wouldn't fight"), the rising threat of neo-isolationism, and the nature of the opposition (Dean Rusk opined that it was so effective because "the Communist apparatus is working all around the world").

Sometimes Graff did raise specific issues. There was the question he asked Walt Rostow, for example: Why hadn't the Soviets supplied Hanoi with surface-to-surface missiles? Because, came the answer, "*We* are exercising restraint." Hanoi's air defenses were the most sophisticated in the world, added Rostow. But American pilots had turned in a "very great performance. . . . We use special weapons so as not to kill civilians."

Graff thus witnessed Johnson's final agonizing over the war. During their last conversation the president rambled on about the "unpatriotic sentiments of the Department of State" and the "cheap press." "The government is infiltrated," Johnson declared at one point, "and the airwaves are infiltrated." But if Townsend Hoopes is right, the biggest covey of doves turned up in the Pentagon, within earshot of Robert McNamara's droning computers. Hoopes

tells the story of how the enemy's 1968 Tet offensive revealed these dissenters to one another. But his book begins with an informative discussion of the Kennedy administration's proposed solution to "wars of liberation" before it moves on to a consideration of the results. Taking proposed solution and results together, as Hoopes has done here, we have a powerful indictment of foreign policy. "We seem to be proceeding on the assumption that the way to eradicate Viet Cong," observed a fellow dove to Hoopes in 1967, "is to destroy all the village structures, defoliate all the jungles, and then cover over the entire surface of South Vietnam with asphalt." The story ends with the now-famous episode in which Clark Clifford transforms himself into the biggest dove of them all, and, with the aid of Dean Acheson and others, persuades the president to order a partial halt in the bombing.

Robert McNamara told Graff in 1968 that he now thought it essential to have a thorough critique of American involvement in Vietnam, going back to the Second World War. How unfortunate for the nation at the time that the secretary of defense took so long in coming to that conclusion; how fortunate for future decision makers that we now have the Pentagon Papers.

LLOYD C. GARDNER
Rutgers University,
New Brunswick

MAGNUS MÖRNER, editor. *Race and Class in Latin America*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1970. Pp. viii, 309. \$10.00.

This volume that Magnus Mörner has carefully edited contributes substantially to historical knowledge of complex topics. The thirteen contributors include historians, anthropologists, sociologists, political scientists, and geographers from various countries. They treat problems that in time range from pre-Hispanic native groups to current immigrations from Asia, Africa, and Europe and in area cover the Caribbean, Mexico and Central America, and the continent of South America. Unlike most collective works of this nature, all the essays are of high quality. One could only wish that space had permitted the inclusion of contributions by

Fernando Cámara Barbachano, Juan Comas, Marvin Harris, George A. Kubler, Seymour Menton, and D. F. Solá.

The papers were the outcome of a conference on race and class in Latin America during the national period organized by Professor Mörner, co-sponsored by the Latin American programs at Cornell and Columbia Universities and held in New York, December 16–18, 1965. The principal aims, as reflected in the present volume, were, first, to focus scholarly attention on topics that have largely been neglected if not ignored; second, to survey the present state of scholarly knowledge regarding race and class; and third, to encourage reinterpretations and reformulations on more solid information. Many current generalizations on these topics are in fact scholarly or popular myths, more often based on outmoded theories and prejudices than on verified data. By assessing the state of the art, the present volume is designed to open up fruitful lines of research in a variety of such related fields as demography, social psychology, cultural and legal history, and historical acculturation.

The disparate approaches are grouped into four parts. Part 1 concerns the abolition of slavery and its aftermath. Gonzalo Aguirre Beltrán's contribution on integration of the Negro into the national society of Mexico, Carlos M. Rama's essay on the passing of the Afro-Uruguayans from a caste to a class society, and Richard Graham's survey of the abolitionist movement in Brazil are all rich in new data that support important reconsiderations. Their findings put in serious jeopardy the prevailing notion that independence was merely a political not a social revolution. In Mexico, for instance, abolition of slavery was part of a general shift from a caste to a class society that changed the social structures of the new republic. The independence movements had mass support from Negroes and Afro-Mexicans who saw in them new routes of social mobility. Much the same situation occurred in Uruguay. Graham highlights the fact that the Brazilian slaves hastened abolition by fleeing in large numbers from the uneconomic colonial plantations. Their dissension, coupled with external pressures from the British, brought an end to slavery.

Part 2 considers immigration, stratification,

and race relations during the nineteenth century in Peru, the Dominican Republic, and the city of São Paulo, Brazil. Mario C. Vázquez shows that immigrants made a considerable impact on Peru, that Negroes were more numerous than anyone had realized, and that the general mixing of races and cultures of the period extended even to native Indian communities, which were previously thought to be closed and impervious. Harry Hoetink's new data on the Dominican Republic emphasize a curious distinction between the "first" and the "second" families and groups and a shifting relationship from one to the other in the political, social, and economic realms. Florestan Fernandes' important contribution on São Paulo works out the complex relationships between the European—largely Italian—immigrants who displaced slave labor, and ex-slaves who as rural then urbanized proletariat had improved neither their social nor economic position as freedmen. It was not until the "Second Industrial Revolution" of the 1930s that Afro-Brazilians began to be absorbed more fully into the society, culture, and economics of republican Brazil. This and other treatments in the volume reject the romantic moonshine voiced by Frank Tannenbaum and others that Brazil was the paragon of racial coexistence. Fernandes also discusses and dismisses the notion that immigrants introduced racial prejudice. It was endemic. The first immigrants who felt its sting finally overcame it.

Nineteenth-century Indo-American societies undergo scrutiny in part 3. Moisés González Navarro reiterates and extends findings he first published in 1954 on Mexican native groups under the republic. When the Indians were stripped of their special colonial ward status, they were subjected as "citizens" to debt peonage and disenfranchised by states on grounds of illiteracy. During most of the nineteenth century native groups were in revolt.

Constant political and military turmoil encouraged miscegenation. The Mexican Revolution of 1910 defined the final transition from a neocolonial caste system to an open society, although as late as 1940 marriage partners were still recorded by race.

Manning Nash focused on what impact the Industrial Revolution and the entrance of

Mexico and Guatemala into the international economy had on Indian communities. Two general patterns emerged. In one the natives in nuclear mountainous enclaves were essentially bypassed. As a defensive stance against the capitalist pressures, they reformulated their inherited cultures to provide a complex system for exchange of goods under hierarchical structures of political and spiritual power that minimized intergroup frictions. In the coastal regions and areas where commercial haciendas produced export goods, Indian cultures were devastated. Natives generally became deculturated peon wage-earners, along with Afro-Mexicans and mestizos of the same economic class.

Somewhat the same message is conveyed by François Chevalier's inquiry into the official 1920s Indianist program in Peru. After nearly a century of considering that Indian communities were obstacles to progress, the 1920 Constitution recognized them as legal entities and made the state responsible for protecting them. Chevalier shows that this sudden shift in view partially came about through the rise to political prominence of various new middle groups in Peruvian society who were more sensitive than previous oligarchs to the misery of highland Indians. President Augusto Leguía capitalized on this sensitivity by promising and securing remedial legislation; once the laws were on the books Leguía abandoned this sponsorship, and for many years allowed the legislation to remain a dead letter. Chevalier points out the paradox: the products—wool, leather, and cattle—that created affluence and the middle groups commercialized Indian lands and thus encouraged increased despoliation. He writes that "the injuries inflicted upon the Indian were, in great part, a consequence of the brutal penetration of a modern economy into the rural world of the highlands."

The final section of the work reports on the state of knowledge about race and class relationships. Charles Anderson notes that while fellow political scientists agree on the importance of class and race, they have no consistent or coherent notion of what the terms mean, and generally are vague or misleading when attempting to relate them to theories about Latin American politics. Octavio Ianni, a Marxist, also attacks the myth of racial democracy in Brazil and surveys studies on the accul-

turation of Indians. Of special interest is his summary of the difficulties that even Brazilian researchers, let alone foreign, face in race-class studies, given the official desire to preserve the myth and to maintain a social status quo. Perhaps the most important essay in part 4 is Mörner's own summary of historical research on race relations in Latin America during the national period. Based on a survey of the literature, he sets up six main priorities for future research. Since this essay, his own *Race Mixture in the History of Latin America* (1967) has appeared, synthesizing much recent and earlier research.

With rising public, popular, and scholarly interest in these topics, *Race and Class in Latin America* is an especially timely and seminal book. Not only should it prove valuable to specialists on Latin America, but to comparative studies where many of its insights and theoretical concepts are applicable. This work is highly recommended.

†HOWARD F. CLINE

JAMES W. WILKIE and EDNA MONZÓN DE WILKIE. *México visto en el siglo XX: Entrevistas de historia oral. Ramón Beteta. Marte R. Gómez. Manuel Gómez Morín. Vicente Lombardo Toldano. Miguel Palomar y Vizcarra. Emilio Portes Gil. Jesús Silva Herzog.* México, D. F.: Instituto Mexicano de Investigaciones Económicas; distrib. by "Cuadernos Americanos," México, D. F. 1969. Pp. x, 770. \$9.00.

A collection of oral histories is very much like life: interesting but not easily amenable to organization. The personal accounts of seven prominent Mexicans provide great insight into the functioning of the Mexican political system and the evolution of political thought since the Revolution. For the reader, however, the task of plowing through the material is lengthy, but he is aided by an analytical index. The seven personalities all played important roles in the shaping of the course of the Mexican Revolution, although not all were working toward the same goals.

In alphabetical order, the interviewees are: Ramón Beteta, politician and treasurer, who held important political positions in five administrations; Marte R. Gómez, agrarian leader and participant in many of the early regimes that defined Mexico's land policy; Manuel

Gómez Morín, a founder of the opposition right-wing Partido de Acción Nacional and a participant in pre-Cárdenas governments and in the drafting of early laws concerning, among other things, income taxes and agricultural credit; Vicente Lombardo Toledano, theoretician and militant Marxist, participant in the formation of many labor organizations, and opposition candidate for the presidency in 1952; Miguel Palomar y Vizcarra, militant Catholic, who played an important role in the religious uprisings of the twenties; Emilio Portes Gil, former president, who shaped agrarian and labor policies and guided Mexico through some early crises; Jesús Silva Herzog, economist and historian, who was influential both as participant in and critical observer of Mexico's history.

As the Wilkies pointed out in the introduction, the value of the book lies principally in the light that these men shed on the events of the 1930s. Although there is little in the way of new information in the comments of these men, the comparisons of their opinions and viewpoints can provide an understanding of the interplay among different approaches to given problems. The book is basically devoted to the study of three topics that appear to have been of interest to the Wilkies and in which all the interviewees played an important role: the Cristero movement of the 1920s, the impact of Cárdenas on Mexico's political life, and the role of communist ideology on the country's political evolution. Evidently other topics were discussed at great length throughout the interviews, but James Wilkie's questions and his strongest personal interventions are directly related to these three topics.

In a short review even the flavor of the book is hard to convey, but the differing approaches to the Mexican Revolution may provide some illustration of what may be found by persistent digging. Of it Ramón Beteta said that "we understand . . . not only social reforms, but also we take as revolutionary acts all those which mean equipping the country with an infrastructure. . . . In England, for example, where these changes have also occurred, they would not be called revolutionary" (p. 32). Marte Gómez noted that the closing of the path to agrarian communism was, from the Mexican point of view, "a recourse to give the *ejidatario* tranquility and stability" (p. 102). Miguel Gómez

Morín recognized that the revolution has social content but argues that much of what has been accomplished "has been done by the Mexican people in spite of the Revolution! Who knows what might have been achieved in Mexico if it were not for what is presently usurped in the name of the Revolution" (p. 215). Lombardo Toledano is unequivocal in stating that the Mexican Revolution was not socialistic and that many things cannot be accomplished in his country because it is capitalistic (p. 297). Palomar y Vizcarra claims that the 1917 constitution was "the work of a few miserable people at the service of North American sectarianism" (p. 437). Portes Gil proudly said that "I had to demonstrate that I was going to create a radically revolutionary government. . . . And fortunately I fulfilled that task" (p. 545). Silva Herzog points out that the revolution was not a bourgeois one: "it was a popular, peasant, nationalistic revolution" (p. 692). But he also quotes an incisive phrase of another Mexican when asked to judge more recent accomplishments: "Incense smells nice but ends up staining the idol" (p. 708).

It is a shame that some of the rich material that the Wilkies collected in their interviews with and written responses from Lázaro Cárdenas were not included in this volume; hints of these discussions appear in this book and excerpts are included in their other writings. It is evident that Cárdenas influenced the selection of many of the people included in this volume.

On balance, these interviews add little to our knowledge of Mexican history. They show the courtesy and willingness of seven Mexican intellectuals and politicians to talk at length with visiting scholars but without either opening themselves to a deeper and possibly critical understanding or leaving the security of their own well-known frames of reference. They do, however, have a heuristic value: the well-informed reader can find suggestions for additional research on political aspects of Mexican history. Finally, while it is probably impossible to conduct a completely "objective" interview, the authors clearly had preconceived hypotheses about the functioning of the Mexican system, which might have been set forth more clearly at the beginning.

DAVID BARKIN
El Colegio de México

Communications

TO THE EDITOR:

Peter Paret, in his review article "Assignments New and Old" (*AHR*, 76 [1971]: 119-26), noted two weaknesses of diplomatic history as it is written today: an unwillingness to devote serious attention to the ideas that stand behind the diplomats and "an equally pervasive refusal to explore the emotional sources and psychological impact of policy." What Paret is suggesting is that the jet-age historian must try to get inside his subject (as Erikson did with Martin Luther), explaining foreign policy in terms of private feelings and political philosophy. My only objection is to Professor Paret's implicit assumption that diplomatists as a rule withhold part of themselves, when, in fact, their personal thoughts and idiosyncrasies are probably better known than the documents they sign, thanks to journalists and to their friends' habit of keeping diaries or notes that usually become books.

THOMAS M. IIAMS
U.S. Embassy, Lisbon

PROFESSOR PARET REPLIES:

My article, which discussed William Langer and the interpretation of foreign affairs, not diplomacy itself, may have reflected a general belief that all sane men as a rule withhold part of themselves, but so far as I can see implied nothing about degrees of personal reserve shown by diplomats.

May I, in turn, point to an implicit assumption in Mr. Iiams' letter with which I would regretfully agree: The quality of work done by most instant historians tends to be on a par with the accomplishments of the jet-age diplomats they write about.

PETER PARET
*Historische Kommission
zu Berlin*

TO THE EDITOR:

My perusal of the *AHR* is rather irregular, but coming across Robert V. Allen's review of B. I. Marushkin's *History and Politics: American Bourgeois Historiography of Soviet Society* (*AHR*, 76 [1971]: 475), I am moved to protest.

Mr. Allen is no doubt correct in his resentment of Marushkin's invectives; in a Soviet book on an American topic they are rather *de rigueur*. But a reviewer of Soviet books who cannot look beyond the smoke screen of vituperation in Soviet writing on the United States does a poor job indeed. What Mr. Allen should have stressed is the other side of the coin: Marushkin's detailed, and not entirely unfair, knowledge of American scholarship on the Soviet Union and on historical subjects in general. I have read the book myself and was rather impressed by the scope of American scholarship that the author manages to convey to his Soviet readers. One wonders sometimes whether there does not exist, behind the vilification, a secret wish to popularize American views of history and of the Soviet Union to Soviet audiences. Being quoted in Marushkin's book almost as frequently as Lenin and George Kennan, I have no objection to seeing my views matched against those of the party, even at the price of some distortion. It seems to me that there are two ways of reviewing Soviet books. Mr. Allen has dealt with only one side, the side that perpetuates prejudice and rancor.

THEODORE H. VON LAUE
Clark University

DR. ALLEN REPLIES:

It would not basically alter the essence of my review of Mr. Marushkin's book to add that, like the curate's egg, "parts of it were excellent," for there is neither optimism nor charity

enough in me to discern any intent on his part to communicate a balanced assessment of American writing on Soviet history. The diligence with which he has read in the field does not compensate for the manner of presenting his argument, flecked as it is with such words as *fal'sifikatsiia* (falsification), *izvrashchenie* (distortion), or *antinauchnaia* (unscholarliness). These words seem so far beyond the bounds of scholarly discussion—especially when it is noted that all acts of the Soviet state are presented with uncritical and glowing praise—that I feel that if there is any rancor in this situation it is not of my creating. From more judicious Soviet scholars than Mr. Marushkin one can accept almost equally hard language, saying to them, in the words of Owen Wister's Trampas, "When you call me that, smile," since one does feel that they are seeking to make their points through reasoned argument rather than vehemence. I do not see any smile in Mr. Marushkin's prose.

ROBERT V. ALLEN
Library of Congress

TO THE EDITOR:

In his article "Cultural Strain and Protestant Liberalism" (*AHR*, 76 [1971]: 386–411), William R. Hutchison puts forth a number of provocative notions. Particularly important is the often ignored relationship between religious ideas and cultural conditioning. In my own research ("American Christian Thinkers and the Function of War, 1860–1920" [Ph.D. dissertation, University of Kansas, 1970]), I was struck by the frequency with which religious leaders reflected cultural values, not pristine theology, in the formation of their views on war. If I understand his argument, Professor Hutchison has vivified James Dombrowski's 1936 thesis that Social Gospelers made the kingdom of God resemble a country club and has reinforced the importance of Walter Rauschenbusch's frequent concerns, at the turn of the century, about the need for clergymen to reassert their leadership over the nation, despite their loss of control over social reform movements.

I am troubled, however, by the ambiguity of several of his ideas. For one thing, Hutchison provides us no clear definition of liberalism. Among "liberals" mentioned in footnote 1, page 407, he includes Charles A. Briggs, who

was liberal in theology, but not in social theory, and G. B. Smith, who with Shirley Jackson Case and Frank H. Foster went far beyond liberalism's reliance on such essentially traditional ideas as divine omnipotence, ethical absolutism, and clerical primacy. Neither does he, for that matter, define conservatism (p. 394). What would he do, for example, with Archibald A. Hodge's strangely "liberal" remark in 1886 about the need to bring about belief in the fatherhood of God and the brotherhood of man? I am convinced that close reading of "conservative" and "liberal" theologians in this period reveals more similarity than Hutchison indicates. Neither group rejected the ideas of the divine control of history, thus relying on the traditional idea of man's limited freedom, or of the need for the Church's domination over society. It is not until one gets to Walter Rauschenbusch and W. D. P. Bliss that one reads ideas such as that the Church is only one of many social agencies (rather than the only and most effective); it is not before the writings of Case, Smith, G. B. Foster, D. C. Macintosh, and Frank Hugh Foster (ca. 1910–20) that one begins to see fundamental changes in theology (that is, "modernism"). It is also clear that most religious leaders in this period were essentially fearful of democracy unless it had ecclesiastical controls; that is, they were heirs of earlier pessimism about Jacksonian America.

Despite the size of his survey, moreover, Hutchison relies, as he admits on pages 410–11, on a highly limited number of subjects—thirty-three. In addition he uses their biographies, not their writings, to discover responses to such issues as party allegiance, civil rights, and World War I. This method is misleading, in large part because few biographies of religious leaders have escaped the tendency to praise their subjects. On the issue of participation in World War I, for example, my study of the writings of these leaders in wartime reveals that, of the eighteen men Hutchison mentions as living in 1917, none except Rauschenbusch was skeptical of the war's justness; some, like Abbott and William DeWitt Hyde, were "reluctant followers of the Wilsonian position" not because they opposed the war but rather because they were followers of Theodore Roosevelt; some, like John Wright Buckham, A. C. Knudson, and Newman Smyth, were silent on the war ques-

tion; and some, like Henry Churchill King, Harry Emerson Fosdick, and George Herron, were avid Wilsonians. Biographies and autobiographies of these men, by contrast, tend to reflect the disillusionment of the interwar period and hence indicate a more critical approach, in retrospect, to the war. On another issue, "attitude toward labor movement," what does it mean when Hutchison indicates that fifty-eight per cent were "highly sympathetic"? Perhaps Rauschenbusch, Bliss, and Harry Ward would qualify as notable sympathizers, but few religious liberals accepted the idea of the labor strike. The tendency was, as Washington Gladden argued in 1887, to defend labor's right to organize but not its right to strike. Religious leaders encouraged cooperative discussion instead. Or, on the issue of Negro rights, what was an "advanced position"? Henry Ward Beecher, mentioned on page 411 as a Black Republican, may have defended emancipation, but he also backed Andrew Johnson's position on equality of the races. So did Phillips Brooks. I would also question where Hutchison found that two-thirds of the fifteen were anti-imperialists. In the writings that these fifteen men produced from 1898 to 1901, I found only Rauschenbusch to be an anti-imperialist. Bishop Henry Codman Potter, not mentioned in the article, was both antiwar and anti-imperialism but moved away from that position in 1899. Washington Gladden was an avid imperialist in 1901 but in 1905 indicated he may have been too optimistic about his country's intentions. How many, in short, were anti-imperialists in 1898? How many became so after the damage had been done? In short, biographical analysis for such men is not as reliable as the analysis of their writings and manuscripts.

In a word, Professor Hutchison has provided a somewhat fresh approach to the problem of religious leadership in the years 1875-1920, but I think he fails to define his subject matter precisely or to indicate why he ultimately relied on so few men, on biographies and not primary sources, and on questions (pp. 410-11) that are misleading.

D. E. BIGHAM
Indiana State University,
Evansville

PROFESSOR HUTCHISON REPLIES:

Since I must disagree with many of Professor Bigham's criticisms, it would be pleasant at least to be able to accept the favorable estimates in his opening remarks. But he has misread the conclusions of the essay. My investigation tended more to refute than to support a country-club image of liberalism. It also modified, without entirely nullifying, the view that the late nineteenth-century clergy had suffered severe loss of prestige.

I am afraid that, on the whole, Mr. Bigham became carried away with his objections to certain statistics given at the end of the appendix and lost nearly all contact with the article itself. His query about Briggs is a case in point. Of course Briggs was a conservative on social reform. The essay and the statistical chart both stressed repeatedly the distinction between those theological liberals who advocated the Social Gospel and those who did not. When Social Gospellers were singled out for special treatment, Briggs was not among those selected.

My investigation could have been based on somewhat different groups of religious liberals and conservatives, and I am sure other scholars will study groups that are defined differently. There was no ambiguity, however, about the way in which these particular samples were selected. We used a criterion of intentionality, which means that we included those who made it clear that they supported, or that they opposed, the so-called New Theology. Our procedure was identical in procedure with the use of membership lists as evidence of adherence to particular ideological positions or configurations. Since membership lists can distort by including nominal adherents and by omitting real ones, the kind of criterion we were obliged to use—expressed or demonstrated support—is not necessarily an inferior one.

Mr. Bigham's problem at this point is that he doubts whether there was any genuine liberal movement to adhere to—whether there were any "fundamental changes in theology"—before the decade 1910-20. On such an assumption we would admittedly have no liberal or conservative factions. We would also have no explanation for the controversies that inflamed American religion between 1875 and 1915 and almost no

explanation for the rise of fundamentalism. But the assumption, I would say, is quite unhistorical. If Mr. Bigham imagines that sentiments favoring the brotherhood of man might make a liberal, "strange" or otherwise, out of the younger Hodge—the scourge of liberalism in the 1880s—then I think he has not given his implied "close reading" to the theological discussions of this period.

What he has read for his dissertation, I take it, are the writings on war. And he has apparently found, as I have, that on questions of war, race, and imperialism (if not on other issues of social reform), there was precious little difference between theological liberals and theological conservatives. The proportions of pacifists and imperialists, for example, are rather similar within each faction. But his inference from this—an inference tinged with indignation that the theological dissenters were not thoroughgoing twentieth-century radicals as you and I would have been—is that the theological differences were historically insignificant. And that, I think, is a position that will not stand up.

It is Mr. Bigham's prerogative to regret that Protestant liberalism was not, theologically, the Free Religious Association. But it was not. It was a more influential and massively important movement whose differences with orthodoxy largely defined the controversial history of the time. Its divergence from the received traditions, on each of the points (such as the "omnipotence" of God) that Mr. Bigham mentions, were sufficient to fuel heresy trials, disrupt a half dozen of the largest denominations, and enable a great many churchmen to know precisely on which side they stood.

Mr. Bigham's careless and rather maddening statement that I relied upon thirty-three subjects instead of 256 is another product of his preoccupation with the figures at the end of the appendix. The latter were indeed drawn from about thirty cases, but they were not, except for the data on conversion and home nurture, used in the article. In the original manuscript the table in question appeared as a footnote to the chart, and I think its status would be clearer had we retained that format. While the summaries of political and social opinion appearing there do draw upon the subjects' published writings as well as upon biog-

raphies, the warning given in the article (how I wish Mr. Bigham had read the article!) applies nonetheless. I suggested that "no group profile drawn chiefly from secondary materials can pretend to do more than propose lines of inquiry." I called for work in the manuscripts.

Once better data are collected I expect we shall still differ on such matters as what constituted a becoming reluctance about going to war in 1917. In the pacifism and anti-Preparedness of some of these men even after the declaration, in the wartime protests of others against jingoism, intolerance, thought control, and the moral failure of the churches, I find "reluctance" about the Great Crusade; Mr. Bigham would not. Though I counted no one an anti-imperialist who supported Philippine annexation, I was not restricting the question to 1898–1900, and therefore included persons (on both sides of the imperialist issue) who were youths in 1898 or were dead by 1898; Mr. Bigham apparently objects to that. I categorized Phillips Brooks, who among other things denounced segregation on the street railways, as "advanced" for the 1860s; Mr. Bigham would hold him to a higher standard. The article clearly meant to encourage questioning of this sort—though not necessarily questioning conducted in Mr. Bigham's somewhat unbuttoned manner.

WILLIAM R. HUTCHISON
Harvard University

TO THE EDITOR:

It is generally not a good idea to respond to critical book reviews, if only for tactical reasons—the reviewer has the last word. Besides, a book should speak for itself to the readers of the review. Still, the review by R. M. Hartwell and Robert Higgs of *The Unbound Prometheus* ("Good Old Economic History," *AHR*, 76 [1971]: 467–74) will have gone to so many readers who have not read and will not read the book that a reply is called for. What is more, the reviewers, writing in a field more or less unfamiliar to many of their readers, have chosen to cloak themselves in an aura of esoteric competence and specialized terminology. A certain amount of demystification is therefore desirable.

Unfortunately the sloppiness of the review makes it hard to do this without first clearing away the debris of error. As the comments that follow make obvious, Messrs. Hartwell and Higgs repeatedly charge me with writing things I haven't written, doing things I haven't done, and not doing things I have done. Their charges, moreover, are usually expressed in elliptical phrases whose meanings are not always clear and ostensibly illustrated by page references that are not always accurate. (There ought to be a way to protect authors from slipshod nit-picking, especially since editors have not yet accepted the principle of equal time.) Thus I allegedly find it hard "to explain the persistence of 'obsolete' [their quotes] technology" (Hartwell and Higgs, 473). Yet I do not even use the word "obsolete" in any of the contexts referred to, though in one passage I do use the word "obsolescent," which is a very different kettle of fish. Similarly, I am accused of ignoring (or of being ignorant of?)—there's a gratuitous swipe!—the work of a number of economists. Yet Robert Solow is represented through the report of the National Commission on Technology, Automation, and Economic Progress, which he helped prepare; I do make extensive use of the volume on *The Rate and Direction of Inventive Activity*, though I do not mis-cite it, as Hartwell and Higgs do, under Richard Nelson, editor; I do discuss Edwin Mansfield's work on the rate of diffusion of innovation (*Prometheus*, 519 n.4); and indeed I make use in general of a great deal of economic literature, especially in the treatment of the more recent periods. Thus, using the kind of statistical criterion that Hartwell and Higgs admire, I find that twenty-six out of thirty-five works cited in the analytical section of the chapter dealing with the post-World War II period (*Prometheus*, 486–513) are by economists. Some of my historian colleagues might suggest that I overdo this sort of thing. To be sure, I may not make the same use of this work that Hartwell and Higgs would (I'll come back to this point), but anything approaching a careful reading of the book would have shown them that I am hardly ignorant or contemptuous of this material, nor, for that matter, of the men who wrote it: every part of the book has been read at one time or other by first-rate economists. Two of the readers of the first part, for example, dealing with

the Industrial Revolution in Britain, were Kenneth Arrow and Robert Solow, whom Hartwell and Higgs would call to my attention; another reader, if memory serves, was Max Hartwell himself. Needless to say, none of these readers is responsible for my failings; but if I have not cited some of their own work, it is because I did not find it helpful in what I was doing or because I did not feel I needed their support for what I was writing.

One thing I could not do was to cite works that appeared from early 1968 on. The book went to press that spring. The most ludicrous of the review's reproachful footnotes are those referring to works not yet published at the time of the review. It's hard enough to keep track of the material in this field without reading minds.

One good point that Messrs. Hartwell and Higgs make is that the book could use charts and maps. It could. Even so, the picture they give of the statistical apparatus is as misleading and erroneous as the one given of the sources used. They state that of 16 tables covering the period of 1914 only one "pertains to the Industrial Revolution in Britain, and only two . . . to the early industrialization of Europe." In fact there are 15 statistical tables in the text, plus a half dozen or more in the footnotes, dealing with the period to 1914; 12 of the 15 text tables contain data on Great Britain; 4 (not 1) of these go back before 1850; 9 (not 1) go back before 1870 (whichever date one prefers as terminus of the British Industrial Revolution); 7 (not 2) deal with pre-1870 development on the Continent. Now it may well be that there should be more of these, although, as Messrs. Hartwell and Higgs point out, there are a great many further numbers imbedded in the text. (They think this a disadvantage. I think that the numbers in the text are the only ones that some readers have looked at.) Still I would argue that people who lay so much stress on statistics as Hartwell and Higgs do ought to get their own statistics right. Messrs. Hartwell and Higgs distinguish between historians who can write and economists who can count (Hartwell and Higgs, 471). Shall we add a third category for those who neither write nor count well?

What of the substantive criticisms of the book? Here I shall have to pass over the nonsense—such as all the innuendoes, directed not

only against me but against historians in general, about lack of definition, or my alleged failure "to see the Industrial Revolution as growth and industrialization interacting simultaneously." I would simply ask Messrs. Hartwell and Higgs to reread (read?) the book, especially the first few pages. Insofar as space permits I should like to focus on that area where I am said to get "into the most trouble"—on technological change. I am charged with assuming, "at least implicitly, that the growth sequence of modern times is that of technological change, industrial development, and economic growth." So what's new? So long as one keeps in mind the feedback effects of growth on innovation—and *The Unbound Prometheus* is full of this—this is a good summary of the sequence of change in the Industrial Revolution, acceptable to the vast majority of economists. To be sure, Messrs. Hartwell and Higgs cite Dale Jorgenson and Zvi Griliches as arguing "that in one sense almost no growth can be attributed to technological change" (Hartwell and Higgs, 472 n.13). But I can only infer from that statement that they do not really understand what Jorgenson and Griliches are saying. Jorgenson and Griliches are not talking about technological change in my sense, or yours, or even Hartwell's and Higgs'; they are talking about a kind of book-keeping concept—a cover word used by economists to denote the variety of unexplained residual factors responsible for growth over and beyond what can be accounted for by increases in capital and labor (as conventionally measured). Insofar as Jorgenson and Griliches can transfer all or part of that unexplained residual to the account of labor or capital, they will have reduced the share of growth imputed to "technological change." But they will not thereby have eliminated technological change (without quotation marks) as a source of growth; it will be found embodied in quality changes of the labor and capital employed to produce goods and services.

What of the alleged Landes theory that technological change is "mainly" (their word) a response to wage increases? Landes, say Hartwell and Higgs, does not recognize that wage increases need lead only to the substitution of capital for labor and not necessarily to technological progress (Hartwell and Higgs, 473). In the first place, I do not at any point attempt

to assign degrees of importance or causative influence to different sources of technological change; nor do I use words like "mainly" in this connection. The time may come when we can make such statements, but it is not yet here. Second, I do believe, as do all the economists I read and talk to, that changes in relative factor costs (an increase in the wage rate as against the cost of capital) do induce manufacturers to substitute capital for labor and that this substitution almost invariably entails technological progress—partly because innovations that are intended to save labor also generally save capital; partly because new equipment usually embodies advances even if it is intended to be no more than a reproduction of machinery already known and in use; and partly because new needs and opportunities stimulate invention. Third, and most important for economic history, the fact is that the effort to save labor in the Industrial Revolution did stimulate technological progress, just as mechanization did lead to growth. (This does not mean that there were no instances of unprofitable mechanization that did not lead to growth. Messrs. Hartwell and Higgs charge me with believing that best-practice technology [presumably they mean "most profitable technology"] is always the most capital-intensive technology. I explicitly make the point that "there can be such a thing as overmodernization—an excessive substitution of capital for labour" [*Prometheus*, 354].)

The pitfall that Messrs. Hartwell and Higgs have fallen into here is one that sometimes afflicts economic historians who have had a small but inadequate dose of economic theory: they become so enthralled by some of the odd possibilities hypothesized by the theorists that they lose touch with reality. One can conceive of the substitution of capital for labor without a gain in overall productivity; one can even draw charts to illustrate the phenomenon. It's just hard to persuade real life to behave that way.

The review further charges me with postulating "necessarily diminishing returns to innovation" (Hartwell and Higgs, 474). I have no such belief, make no such statements, and indeed devote much of the latter part of the study to the opposite proposition. I do say, however, that given areas of technological advance show diminishing returns, which is a much more

limited thesis. It says, for example, that once the self-actor and ring frame were invented further improvements in spinning machinery would yield relatively smaller gains. Like all predictive theses this one is intrinsically uncertain, if only because man's capacity to invent new ways of doing things is very great, and who can say what new method of spinning he may yet invent? It also poses problems of definition: when is an innovation an extension of an earlier line of advance, and when new? Still, many men before me, including some of the economists recommended by Hartwell and Higgs, have noted the tendency of given branches of industry to slow with age, owing in part to technological maturity. (Compare Simon Kuznets, "Retardation of Industrial Growth," *Journal of Economic and Business History*, 1 [1929]: 534-60; W. E. G. Salter, *Productivity and Technical Change* [Cambridge, 1960], 133 ff.)

Reading the review it is hard for me to understand how Hartwell and Higgs visualize the workings of innovation and technological change. They question the existence of discontinuities: "Such measurements of the movement of total factor productivity as we have show no such breaks. . . ." Do they believe that the course of innovation, like that of true love, runs smooth? If so, they aren't reading the same economists I am; they're not even reading the ones cited in their footnotes. Almost all efforts to measure total productivity show an increase in the rate of technological change during the period after World War I as compared with that before; and the rate varies with such cyclical and intercyclal variables as investment. (Compare Robert Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics*, 39 [1957]: 312-20; Edwin Mansfield, *Economics of Technological Change* [New York, 1968], 27; Zvi Griliches, "Agriculture: Productivity and Technology," *International Encyclopedia of the Social Sciences* [New York, 1963], 242; William Fellner, "Measures of Technological Progress in the Light of Recent Growth Theories," *American Economic Review*, 57 [1967]: 1093-95.)

Please bear with me for one more correction: I am taxed with "the assertion of entrepreneurial stupidity in various countries." The unsuspecting reader of the review will look in vain in *The Unbound Prometheus* for any reference

to entrepreneurial "stupidity." He will find, however, a number of references to entrepreneurial irrationality (in the technical sense of not maximizing profits), which is something very different. Messrs. Hartwell and Higgs are no doubt projecting their own value system when they describe such behavior as "stupid." There remains, of course, the substantive issue: do Hartwell and Higgs believe that entrepreneurs are never irrational, that over time industry is always using the optimum, that is, profit-maximizing techniques? Economists are less confident. Thus Salter on the principle of sunk costs, which Hartwell and Higgs pretend to understand: "Its application to questions of obsolescence and the life of capital goods has been honoured more in the breach than in the observance" (*Productivity and Technical Change*, 61).

Behind these criticisms of *The Unbound Prometheus* there is, of course, a broader attack on the character and purpose of what the reviewers (scornfully? sarcastically?) describe as "good old economic history." What should be the function of the field: to test economic theory against the facts of economic experience, as Hartwell and Higgs suggest, or to study the economic aspects of change and to integrate them into the larger historical context? Or both? Why not both?

It has to be both if economic history is to thrive. And this is what is most disturbing about the review, which tries to proclaim a new orthodoxy while driving the heretics out of the fold. It combines a tender sensitivity to alleged offenses against the virtues and achievements of economic theory (it is hard to see how such sun worshipers are ever going to perform the critical function they arrogate to themselves) with an arrogant contempt for the alleged methodological backwardness of the historian. The reader who cares to track down the pages cited as evidence of my "hostility" (their word) to economic theory (*Prometheus*, 357, 510, 520, 525) will find nothing of the sort. (In one instance [p. 520] he will find nothing even bearing on the subject.)

The Unbound Prometheus does, however, make the point that the approaches of the economic theorist and the historian to reality are different, that where the one tries to simplify, the other tries to seize reality in all its com-

plexity. Both goals are will-o'-the-wisps: simplification necessarily changes and even distorts, though if one keeps in mind the nature of the changes and distortions (what the economist calls the restrictive assumptions), simplification is an avenue to understanding. On the other hand, the whole truth is unattainable; even so, certain kinds of understanding are possible only if one strives to attain them. These are not the observations of a historian who seeks to put down economists (they are un-put-down-able). The best economists themselves are the first to point out the limitations (as well as the strengths) of their own work.

Whence, then, this high dudgeon, these invidious references to "a skillful distillation of the now considerable literature by historians [only historians?] on industrialization," to the backwardness of historians in defining concepts or specifying problems, and to "a quite different kind of comparative economic history" (Hartwell and Higgs, 469, 467, 469)? If the his-

torical profession, to say nothing of David Landes, were to take Messrs. Hartwell and Higgs at their word, it would sink into a deep depression of inferiority.

Fortunately things are not so bad as the article makes out. There is still room for different kinds of economic history—even good old economic history of the kind that Max Hartwell and I have been turning out. Let the historians beware: Messrs. Hartwell and Higgs are not economists; the quality of their criticisms makes that obvious. They are the camp followers of economics, trying to earn themselves some kind of membership by being more papist than the pope. It will take more than an incompetent review to turn the trick.

DAVID S. LANDES
Harvard University

A reply from Professors Hartwell and Higgs will be published in a subsequent issue.

Recent Deaths

Death came on May 5, 1971, to GEORGE L. ANDERSON, professor of history and former chairman of the department of history at the University of Kansas. Born in Kansas in 1905, he spent the greater portion of his career in his native state but was widely known throughout the nation. He received his first two degrees from the University of Kansas and completed his Ph.D. at the University of Illinois in 1933 under the supervision of James G. Randall. Beginning a lifelong interest in banks in graduate work, he published several articles on banking and currency in the last half of the nineteenth century and in 1968 issued *The Widening Stream: The Exchange National Bank of Atchison, 1858-1968*.

His first appointment to a college faculty was at Colorado College, where he served from 1934 to 1945. Developing a second important research interest in railroads, he turned his attention to the Colorado scene and completed one of his major publications, *General William J. Palmer: A Decade of Colorado Railroad Building, 1870-1880* (1936), a study that was reissued in 1963 as *Kansas West: An Epic of Western Railroad Building*.

Returning to his native state in 1945, he joined the department of history of the University of Kansas and in 1949 began his nineteen years as its chairman, supervising the increase of the department from eight to forty faculty members. His interests in railroads continued and resulted in *Four Essays on Railroads in Kansas and Colorado* (1971). He also developed a third major research area in the administration of the public land system and completed *Essays on the Public Lands: Problems, Legislation, and Administration* (1971).

Throughout his career George Anderson

searched for satisfactory definitions of the nature and purpose of history. His students were forced to consider these questions, and he wrote a number of essays that are now available in *Variations on a Theme: History as Knowledge of the Past* (1970).

He exercised leadership at all levels in organizations interested in history. At the national level he served from 1955 to 1957 as chairman of the executive committee of the Mississippi Valley Historical Association, as member of the editorial board of this association from 1957 to 1960, and as president of the Agricultural History Society in 1956.

At the state and local levels he was also very active, for he understood the real significance of local history and its place within the larger frameworks of regional and national history. A member of the board of directors of the Kansas State Historical Society for many years, he served as the society's president in 1961. He was also president of the Kansas History Teachers Association in 1951 and the Douglas County (Kansas) Historical Society in 1967. Other important contributions were made to state and local history through his work with the Cultural and Heritage Arts Center in Dodge City, Kansas; the American Association for State and Local History; and the American History Research Center of the State Historical Society of Wisconsin.

From 1955 to 1957 he organized and directed three historical conferences at the University of Kansas for invited scholars. The third conference yielded a volume of essays under his editorship entitled *Issues and Conflicts: Studies in Twentieth Century American Diplomacy* (1959; reprinted in 1969). A fourth conference at the University of Kansas in 1969 on the trans-Missis-

issippi West honored him for his nineteen years as department chairman; ten papers by leading scholars presented at the conference have now been published as *The Frontier Challenge: Responses to the Trans-Mississippi West* (1971).

Anderson had an enviable reputation in the profession. One faculty colleague described him as "a remarkable man of stability and complete honesty"; another referred to the "total integrity of the man and his scholarship" and to his "keen sensitivity to the rights of other people." One of his graduate students noted that he considered "research and teaching as inseparable parts of the same basic activity," and that "his greatest gift as a teacher was his ability to stimulate interest in history and historical research." What better tribute could be paid to one as a scholar-teacher!

W. STITT ROBINSON
University of Kansas

FOSTER RHEA DULLES died in Jamaica, Vermont, on September 11, 1970. Born in Englewood, New Jersey, in 1900, he took his A.B. degree from Princeton in 1921. He began his career as a journalist, representing the *Christian Science Monitor* in Peking; he was later on the staff of the Paris edition of the New York *Herald Tribune*. After receiving his Ph.D. from Columbia, Professor Dulles taught at Smith College and later at Swarthmore. He was appointed professor of history at Ohio State University in 1941 and served in that capacity until his retirement in 1965. From 1953 to 1958 he was chairman of the history department. Professor Dulles' publications included *Labor in America* (1949; rev. ed. 1955), *America's Rise to World Power* (1955), and *The United States Since 1865* (1959). In addition to his teaching he visited many campuses as a Phi Beta Kappa lecturer and took part in cultural exchange programs in Austria, the Soviet Union, India, and Japan.

Latin Americanists in the United States suffered a grievous loss when HOWARD CLINE, director of the Hispanic Foundation in the Library of Congress, died suddenly of a heart attack on June 1, 1971, cutting short a distinguished career as a scholar, as a promoter and organizer

of projects, and as the administrator of a leading cultural institution.

Howard Francis Cline was born in Detroit in 1915. Overcoming the difficulties of a depression-era boyhood in Indiana, he early exhibited his characteristic industry, determination, and quick intelligence. He earned his way through Harvard College, graduating in 1939 *summa cum laude* in history. A fellowship spent in Mexico opened the way to a later field study of the Chinantec people of Oaxaca and to a lifelong enthusiasm for the Mexican people and their history. Cline's graduate studies, also at Harvard, included both history and anthropology, a happy combination of disciplines that enabled him to approach his life work with an unusual breadth of view. He received the doctorate in 1947, and after teaching assignments at Yale and Northwestern he came to the Library of Congress in 1952. The Hispanic Foundation developed notably under Cline's direction, continuing activities already under way (such as the annual *Handbook of Latin American Studies*) and initiating others. The foundation came to be, increasingly, a national center for all those concerned with Hispanic American studies, and Cline became a sort of informal coordinator of scholarly activities relating to Latin America. He became a valued adviser to government agencies, learned societies, and foundations. He played a leading role in the formation of the Latin American Studies Association, and for some years he was the national member for the United States of the Commission on History of the Pan American Institute of Geography and History. The members of the Conference on Latin American History owe Howard Cline a special debt for his leadership in guiding that organization from a relatively innocuous to a strong position during the 1960s.

Versatility was one of Cline's outstanding characteristics. He was at home in specialized studies of preconquest and colonial Mexico; he was an authority on the Caste War in Yucatan in the mid-nineteenth century; and he was equally capable of outstanding work of a more general nature, exemplified in his two highly valued books on modern Mexico: *The United States and Mexico* (1953) and *Mexico: Revolution to Evolution* (1962). He was also active as an editor: he helped to promote the monumental *Handbook of Middle American Indians*,

which is now in course of publication, and he edited the volumes in this work that deal with ethnohistory. Cline was also the prime mover in the planning, financing, and organizing of a cooperative guide to Latin American historical literature that is scheduled for publication in 1971. It seems almost unbelievable that in addition to all his other varied activities he was able to produce in 1966 a prize-winning study of an early Mexican pictorial document, "The Oztoticpac Lands Map of Texcoco, 1540," which appeared in the *Quarterly Journal of the Library of Congress* in 1966, and that he left almost completed a study of the ancient Mexican calendar that his wife, who was a collaborator in his research, and one of his friends, also a Mexicanist, plan to publish.

Cline traveled widely as representative of the library and of the Hispanic Foundation. His activities were known and appreciated by Latin Americanists far and wide—from Mexico to Argentina and from Spain to Russia. He was one of the first in the United States to recognize the need to keep in touch with Soviet scholarship in the Latin American field and was responsible for the publication of an extensive bibliography of Soviet Latin Americana by the Library of Congress. It was also through his initiative that the *Handbook of Latin American Studies* began to receive contributions from Japan.

Cline was a man of strong opinions. He found it difficult to hide his views behind equivocal statements and could be severe in his criticism of work he considered sloppy or pretentious, but few if any of his colleagues questioned his disinterested zeal for improvement in the quality and range of Latin American studies. His pleasure and pride in the accomplishments of younger men who set new standards of excellence was heartwarming. He strongly disapproved of narrow overspecialization in graduate studies of Latin American history because he felt that it led to parochialism and cut off Latin Americanists from the main currents of historical scholarship and from the great historians of past times. In this respect he anticipated a present trend.

This was the public man whom it will be difficult to replace. For many people, young and old, both in this country and abroad, the loss of a congenial and generous friend will be even more deeply felt. The memory of his good

spirits and optimism chastened by common sense will long endure.

CHARLES C. GRIFFIN
Vassar College

PHILIP MAY HAMER, historian and servant of historical scholarship, died at his home in Bethesda, Maryland, on April 10, 1971. He was seventy-nine, having been born in 1891 at Marion, South Carolina, into a family that united strains of Carolina lowlands and Maine blood. This union prefigured Hamer's concern with the historical records of all sections of the United States, although his strongest ties of sentiment remained with the South and were illustrated and intensified by his choice of a final task—the editing of *The Papers of Henry Laurens*, the first modern, comprehensive work to be devoted to an early statesman of the Deep South.

Hamer earned his bachelor's degree at Wofford College in 1912, his M.A. at Trinity College (now Duke University) in 1915, and his doctorate at Pennsylvania in 1918 with a dissertation on *The Secession Movement in South Carolina, 1847-1852* (1918). Awarded the Harrison Fellowship for postdoctoral studies, he declined it in order to enter military service and was assigned to the Historical Branch of the Army War College.

From 1920 to 1935 he served on the history faculty of the University of Tennessee at Knoxville. Though he performed important administrative duties during those years, they were also years of productive scholarship. His *Tennessee, a History* (1933) is still an essential work; he published in both national and local journals numerous articles and documents relating to Southern colonial and revolutionary history; and in 1926 he helped found and later served as editor for the East Tennessee Historical Society. He also helped found the Southern Historical Association in 1935 and became its president three years later.

In 1935 Hamer was one of the gifted and energetic group of scholars who joined archivist R. D. W. Connor to form the original professional staff of the National Archives. Their contributions were to be remarkable for breadth, vision, and effectiveness in serving both the government and the learned community.

Hamer was in the forefront, holding a succession of highly responsible posts that offered unprecedented opportunities to bring the massive but neglected records of the nation into order for use. He took part in the initial survey of federal records in Washington and was then appointed national director of the Survey of Federal Archives, a project that resulted in an extensive series of published guides. Thereafter he was successively chief of the library, accessions, and reference divisions at the Archives. As director of records control, 1944-51, he and his staff prepared the *Guide to the Records in the National Archives* (1948), still the indispensable key to the more than eight hundred thousand cubic feet of documents then in the National Archives building. There followed in 1950 his two-volume *Federal Records of World War II*, perhaps the first guide ever attempted for such recent records.

A greater mission lay just ahead. In 1950, inspired by the publication of the first volume of Julian P. Boyd's edition of *The Papers of Thomas Jefferson*, President Truman urged the National Historical Publications Commission to submit plans for editing and publishing the papers of other American leaders in the same comprehensive way, with government encouragement but without government subsidization. The commission, of which the archivist is *ex officio* chairman and Hamer was then secretary, was at that time wholly a "paper tiger" since it was without either funds or working staff. But under the Federal Records Act of 1950 it was reconstituted and vitalized, and in the following year Hamer was named its first executive director. The planting and harvesting of documentary works in American history ever since derive in large measure from Philip Hamer's vigorous and imaginative leadership during his eleven-year term of service. The commission's report to the president in 1954, entitled *A National Program for the Publication of Historical Documents*, outlined plans developed by Hamer not only with the members of the commission but with scholars and learned bodies throughout the country. It was proved to be one of the most seminal statements of its kind ever issued. Universities, university presses, libraries, philanthropic foundations, historical organizations of all sorts and in all parts of the country, state agencies, the Congress, and suc-

cessive presidents of the United States supported the effort in their several ways. A printed tribute presented to Hamer in 1960 by the editors whose work he had aided observed: "A decade ago this program was a blueprint. It is now a reality." And in an appreciative article in the *New York Times Book Review* in the same year, Adrienne Koch concluded: "Perhaps most important, Mr. Hamer has organized these projects so that jointly and cumulatively they form an integrated national program, largely carried out under independent auspices and means. The primary result . . . will be to present, more completely and reliably than any earlier age would have dreamed to be possible, a picture of the American enlightenment and of the efforts that launched the American experiment in democracy."

One of the recommendations made in the 1954 report Hamer undertook to carry out himself. This resulted in *A Guide to Archives and Manuscripts in the United States* (1961), a 775-page, indexed compendium of information about the manuscript sources held by some 1,300 repositories in all the states and territories. The project took time and, of course, had lacunae, for many of the institutions, which had to furnish the descriptions of their own holdings, had imperfect information or none at all to impart. They had to begin more or less from the beginning, and it was Hamer who prompted them to do so. When the *Guide* appeared there had never been anything remotely like it for sheer serviceableness in its field of coverage; certainly no individual effort will match it in the future.

In 1940 Hamer married Elizabeth Edwards, then a member of the National Archives staff and now the charming and greatly respected assistant librarian of Congress.

On retiring from his directorship in 1961 Hamer took up the formidable but congenial task of gathering and editing the papers of Henry Laurens, the Charleston planter, president of the Continental Congress, 1777-78, and peace commissioner at Paris in 1782. Two volumes of a projected twelve were issued by the University of South Carolina Press (1968-70), more than enough to indicate the reverence and precision with which Philip Hamer was performing this scholarly labor as he had so many others. For to him, as to Thomas Jefferson, whose words Hamer used as the title of

his presidential address to the Society of American Archivists in 1961, "authentic Documents tending to elucidate our History" are among the most precious possessions of a civilized people. His whole professional life was dedicated to this proposition, and his career dramatically illustrates the interdependence of two high callings, that of the historian and that of the archivist.

L. H. BUTTERFIELD

Massachusetts Historical Society

GEROID TANQUARY ROBINSON, who died in New York City on March 30, 1971, at the age of seventy-eight, was one of a handful of American scholars in the decades before the Second World War to develop a serious professional interest in Russian history. A native of Virginia, he attended Stanford, served overseas as a lieutenant in the First World War, and subsequently received his master's degree and his doctorate from Columbia University, with which he was to be associated from 1924 until his retirement as Seth Low Professor of History in 1960. Eminent in research, teaching, and scholarly administration, his career has had a major impact on Russian and Soviet studies in this country.

At first glance the basis for his impact on these areas would appear to be surprisingly narrow. Robinson was not a prolific writer, and one suspects that his critical sense and sharp editorial eye did not make formal writing easy for him, although he was a masterful letter writer. But he did produce one authentic classic in the field of Russian history. *Rural Russia Under the Old Regime*, which appeared in 1932, remains an indispensable study of the Russian agrarian question and rural society in the half century between the emancipation of the serfs and the outbreak of the Revolution of 1917. Based upon exhaustive archival research and extensive village exploration at a time when this was still possible, the book is a marvel of information, nuance, and insight, presented in a graceful and unmistakable personal style. One of our great losses is that the direction of his later career prevented the appearance of the intended sequel on the peasant world in revolution.

As a teacher, more especially as a sponsor of doctoral dissertations, Robinson had an awe-

some reputation. The band of graduate students who survived the exacting requirements of his sponsorship was not large, but it was devoted: for many, working under GTR was their central educational experience. Legends abounded concerning his ability to write critical notes equaling the length of the submitted draft. He was demanding in his concept of a dissertation, an enterprise he did not regard lightly. He was severe in imposing limits, yet insisted that the subject be important and vital.

Above all he demanded the application of critical intelligence. Most revealing is a passage in his "General Suggestions Respecting Dissertations," a brief but invaluable piece that warrants more than mimeograph circulation. Among other things, he comments on the problem of evidence: "If the evidence on each topic is handled in the manner suggested in Sections 6 and 7, undoubtedly the candidate will sometimes discover what appear at first to be gaps, obscurities, inconsistencies, and contradictions in the evidence. In each case, it is his obligation to do one of two things: (a) study the sources more thoroughly, and clear up the difficulty if that is possible; (b) if the difficulty cannot be cleared up, point out the gap, obscurity, inconsistency, or contradiction to the reader. If the candidate fails to do either 'a' or 'b,' the reader must believe that the candidate has failed to see certain difficulties in his sources, or has tolerated gaps, contradictions, etc., in his own thinking." It is impossible to weigh precisely the effect of the presence of an intellectual scrupulousness such as this in the midst of a somewhat inchoate but rapidly burgeoning field of study, but one feels that the direct and indirect influence was not inconsiderable.

During the Second World War Robinson took leave from Columbia to become director of the USSR Division, Research and Analysis Branch, of the Office of Strategic Services. Here his activities as administrator and director of research were largely out of the public domain, but they had two very important consequences for his postwar career and for Russian and Soviet studies in the United States. First, his wartime experiences early confirmed his sense of the growing importance of the Soviet Union on the world scene. As early as 1943 he was urging upon Columbia the need for establishing a center of Russian studies. Immediately after

the war—in 1946—he was the founder and first director of Columbia's Russian Institute, which became a leading center for advanced research and training. Second, as with other scholars who worked in the OSS, Robinson was struck with the results that could be obtained by bringing the talents of scholars of different academic disciplines to bear on a particular problem. From this setting came a significant part of the impulse toward multidisciplinary area studies, which became such a prominent feature in higher education in the next two decades.

As for Robinson's own interests, the war shifted his concern from the peasant to Soviet social thought, to which much of his energy was devoted in the fifteen years before his retirement. In some respects there is a tragedy in this shift. In contrast to his obvious love for rural Russia and its inhabitants, Robinson approached Marxist-Leninist revolutionary theory

with grimness, a feeling of imperative duty, but no warmth. Although this concern was productive of numerous studies by his students, Robinson himself did not complete a major work in this area.

In many respects he was an austere man, courtly in dress and manner, absolutely unyielding on points of principle or academic standards. But there was also another side, sometimes less apparent: a high sense of the comic and an almost piratical streak. He greatly relished jokes and antic situations. He was remarkably ingenious in devising ways to get around the stupidities of bureaucracies, whether governmental or academic. Finally, he was a man with a passionate love for beauty—in nature, in architecture, in painting, and in the opera. This was Jerry Robinson.

HENRY L. ROBERTS
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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

CH'EN, JEROME, and TARLING, NICHOLAS, editors. *Studies in the Social History of China and South-east Asia: Essays in Memory of Victor Purcell (26 January 1896–2 January 1965)*. New York: Cambridge University Press. 1970. Pp. vii, 423. \$14.50.

SYBILLE VAN DER SPENKEL, V. W. W. S. Purcell: a Memoir.

China: P. CAVENDISH, Anti-imperialism in the Kuomintang 1923–8. JEROME CH'EN, The origin of the Boxers. JACK GRAY, The high tide of socialism in the Chinese countryside. ALASTAIR LAMB, The Sino-Indian and Sino-Russian borders: some comparisons and contrasts. OWEN LATTIMORE, Unpublished Report from Yen-an, 1937. JAMES MACDONALD, The Use of Slogans and "Uninterrupted revolution" in China in the early part of 1964. JOSEPH NEEDHAM, F. R. S., and LU GWEI-DJEN, The Optick Artists of Chiangsu.

South-east Asia: W. E. CHEONG, Canton and Manila in the Eighteenth Century. CHIANG HAI DING, Sino-British Mercantile Relations in Singapore's Entrepôt Trade 1870–1915. GRAHAM W. IRWIN, The Dutch and the Tin Trade of Malaya in the Seventeenth Century. ANTHONY REID, Early Chinese Migration into North Sumatra. KENNETH ROBINSON, Revolution in Education. KERNIAL SINGH SANDHU, Sikh Immigration into Malaya during the Period of British Rule. NICHOLAS TARLING, The Entrepôt at Labuan and the Chinese. WANG GUNGWU, China and South-East Asia 1402–1424.

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[Osteuropa-Institut an der Freien Universität Berlin, Historische Veröffentlichungen, Nummer 14.] Wiesbaden: Otto Harrassowitz for the Institut. 1969. Pp. 237.

FRANK KÄMPFER, Die Eroberung von Kasan 1552 als Gegenstand der zeitgenössischen russischen Historiographie. JACK M. CULPEPPER, The Legislative Origins of Peasant Bondage in Muskovy.

Medieval India: A Miscellany. Volume I. [Centre of Advanced Study, Department of History, Aligarh Muslim University.] New York: Asia Publishing House. 1969. Pp. vii, 316. \$12.00.

SYED HASAN ASKARI, Material of Historical Interest in I'jaz-i Khusravi. IQTIDAR ALAM KHAN, The Mughal Court Politics During Bairam Khan's Regency. SATISH CHANDRA MISRA, The Sikka and the Khutba: A Sher Shahi Experiment. AHSAN JAN QAISAR, Shah-baz Khan Kambu. IRFAN HABIB, The Family of Nur Jahan During Jahangir's Reign: a Political Study. M. ATHAR ALI, Provincial Governors Under Aurangzeb—An Analysis. ZAHIRUDDIN MALIK, Kahn-i-Dauran, The Mir Bakhshi of Muhammad Shah. S. NURUL HASAN, Three Studies of Zamindari System. ZAMEERUDDIN SIDDIQI, The Institution of the Qazi under the Mughals. R. NATH, Glazed-Tile Decorations and Chini-Ka-Rauza. JAGAT VIR SINGH AGRE, Use of Intoxicants in Medieval Rajasthan. SYED MUHAMMAD RAZA NAQVI, Shah Abbas and the Conflict Between Jahangir and the Deccan States. NAZIR AHMAD, editor, Letters of the Rulers of the Deccan to Shah Abbas of Iran. K. A. NIZAMI, editor, Some Documents of Sultan Muhammad bin Tughluq.

La Méditerranée de 1919 à 1939: Actes du Colloque organisé par le Centre de la Méditerranée moderne et contemporaine (Nice, 28–31 mars 1968). [Bibliothèque Générale de l'École Pratique des Hautes Études, VI^e Section.] Paris: S.E.V.P.E.N. 1969. Pp. 176. 36 plates. 37 fr.

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vu par la presse quotidienne de Paris. SALAH EL AKKAD, L'Idée de panarabisme dans le Maghreb. ANNIE REY-GOLDZEIGUER, Quelques témoignages pour une étude du parti communiste algérien de 1934 à 1937. EDWARD SZYMANSKI, Problèmes de l'Afrique du nord et de l'Égypte dans la littérature et le journalisme polonais de la période de l'entre-deux-guerres. ENRICO DE LEONE, Alcune verità storiche sul petrolio della Libia. ANDRÉ MARTEL, Question libyenne et fascisme (1919-1939). PHILIP M. H. BELL, British Policy in the Mediterranean, 1919-1939. B. KOJIC, Le Dépeuplement de la côte adriatique méridionale yougoslave et les migrations rurales. J. HERMITTE, Circulation et croissance dans les Alpes-Maritimes. ANTONIO LOPEZ GOMEZ, Los planes de riegos en Valencia en el periodo de entreguerras (1919-1939). ALFONSO CUCÓ I GINER, Le Mouvement nationaliste en Catalogne (région valencienne et baléares). A. F. MANNING, Le Saint-siège et la guerre civile espagnole. ANTOINE OLIVESI, Les Corses dans l'expansion française contemporaine. DEMOSTHENES SAVRAMIS, Die religiösen Grundlagen der neugriechischen Kultur. H. LADENDORF, L'Âge d'or, retrouvé par la peinture du XX^e siècle sur la côte de la Méditerranée.

Mélanges d'histoire du XVI^e siècle offerts à Henri Meylan. (Travaux d'Humanisme et Renaissance, Number 110.) Geneva: Librairie Droz. 1970. Pp. 195.

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critica di filosofi della storia tedeschi del nostro tempo). KARL PIVEC, Kritische Behandlung mittelalterlicher Texte (Esame critico delle fonti medioevali).

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(Polybios 3, 22, 5). OSWALD SZEMERÉNYI, Si parentem puer verberit, ast olle plorassit. FRITZ SCHACHERMEYR, Damon. JOHANNES FRIEDRICH, Nochmals die phönizische Inschrift von Pyrgi. KARL OLZSCHA, Die Schlußsätze der beiden etruskischen Inschriften von Pyrgi. HEINZ HAPP, Die *Scala naturae* und die Schichtung des Seelischen bei Aristoteles. GERHARD DOBESCH, Zur Philia im Korinthischen Bund. JOACHIM REHORK, Homer, Herodot und Alexander. HILDEBRECHT HOMMEL, Das Doppelgesicht auf den Münzen von Istros. JACQUES HEURGON, Oinarea-Volsinii. HELMUT RIX, Etruskisch *aiseras*. FRANZ F. SCHWARZ, Daimachos von Plataiai. FRIEDER MELLINGHOFF, Zum Dornausziehermotiv. JÓZEF WOLSKI, Arsakiden und Sasaniden. ROCH KNAPOWSKI, Probleme der römischen Chronologie. ULRICH UNGER, "Graunabel" oder: Eine altchinesische Streitaxt und ihre Problematik. HANS DREXLER, Arcana der Iambenkürzung. GERHARD RADKE, Die territoriale Politik des C. Flaminius. GUSTAV ADOLF LEHMANN, Die Endphase des Perseuskrieges im Augenzeugenbericht des P. Cornelius Scipio Nasica. ROBERT WERNER, Die gracchischen Reformen und der Tod des Scipio Aemilianus. HANS ERICH STIER, Der Mithridatesbrief aus Sallusts Historien als Geschichtsquelle. EGON MARÓTI, Aere perennius. HANS OPPERMANN, Späte Liebeslyrik des Horaz. JEAN BÉRANGER, Remarques sur la Concordia dans la propagande monétaire impériale et la nature du principat. PAUL MORAUX, Eine Korrektur des Mittelplatonikers Eudoros zum Text der Metaphysik des Aristoteles. RIGOBERT CÜNTHER, Kolonen und Sklaven in der Schrift *de re rustica* Columella's. IMRE

TRENCSENÝI-WALDAPFEL, Das Rosenmotiv außerhalb des Eselromans. PETER NAGEL, Die Parabel vom klugen Fischer im Thomasevangelium von Nag Hammadi. A. M. MANDELŠTAM, Archäologische Bemerkungen zum Kuschana-Problem. CONSTANTIN DAICOVICIU, Die Romanisierung Daziens. LUDWIG BUDDE, YHIEP THN NEIKHN TQN KYPIQN CEBAC-TQN. P. LAMBRECHTS, and R. BOGAERT, Nouvelles données sur l'histoire du christianisme à Pessinonte.

WEHNER, FRIEDRICH, editor. *Idee und Wirklichkeit in Iberoamerika: Beiträge zur Politik und Geistesgeschichte*. [Institut für Iberoamerika-Kunde.] [Hamburg:] Hoffmann und Campe. 1969. Pp. 131. DM 25.

RUDOLF GROSSMANN, Das Erbe der Mönche und Conquistadoren. FRIEDRICH WEHNER, Der Konflikt zwischen spanischer und liberaler Staatsauffassung in Hispano-Amerika. INGE WOLFF, Desintegration und Staatsbildung in Hispanoamerika, 1810–1840. GÜNTER KAHLE, Historische Bedingtheiten der Diktatur in Lateinamerika. ADOLF MEYER-ABICH, Der Caudillo in seiner historischen Gestalt. GÜNTER KAHLE, and FRIEDRICH WEHNER, Das Militär in der Politik Lateinamerikas. O. CARLOS STOETZER, Die geistigen Grundlagen der spanischamerikanischen Unabhängigkeit. PETER SCHENKEL, Die ideologische Herausforderung Lateinamerikas durch die cubanische Revolution.

Other Books Received

Books listed were received by the *AHR* between May 1 and August 1, 1971. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

ALLARDYCE, GILBERT (ed.). *The Place of Fascism in European History*. Spectrum Book. Englewood Cliffs, N. J.: Prentice-Hall. 1971. Pp. viii, 178. Cloth \$5.95, paper \$2.45.

AYDELOTTE, WILLIAM O. *Quantification in History*. Addison-Wesley Ser. in History. Reading, Mass.: Addison-Wesley. 1971. Pp. x, 181. \$2.50.

BANNOCK, GRAHAM. *The Juggernauts: The Age of the Big Corporations*. Indianapolis: Bobbs-Merrill. 1971. Pp. xii, 363. \$8.50.

BECKER, THEODORE L. (ed.) *Political Trials*. Indianapolis: Bobbs-Merrill. 1971. Pp. xvi, 255. \$7.50.

BLOCK, JACK. *Understanding Historical Research: A Search for Truth*. Glen Rock, N. J.: Research Publications. 1971. Pp. 146. \$4.50.

BRADY, HALDEEN. *Mexico and the Old Southwest: People, Palaver, Places*. Kennikat Press, National University Publications, Ser. in American Studies. Port Washington, N. Y.: National University Publications, Kennikat Press. 1971. Pp. xi, 229. \$12.50.

BRINTON, CRANE, et al. *A History of Civilization*. [Vol. 1.] *Prehistory to 1715*; [Vol. 2.] *1715 to the Present*. 4th ed.; Englewood Cliffs, N. J.: Prentice-Hall. 1971. Pp. xvi, 575, xvii-xxxii; xvi, 578-1200. \$10.95 each.

BROOKS, G. R. *A Select List of Aids of Use in the Teaching of Recent History*. Teaching of History Ser., No. 32. London: Historical Association. 1971. Pp. 61. 30p.

BROWN, DELWIN, et al. (eds.). *Process Philosophy and Christian Thought*. Indianapolis: Bobbs-Merrill. 1971. Pp. xiv, 495. \$15.00.

COLEMAN, WILLIAM. *Biology in the Nineteenth Century: Problems of Form, Function, and Transformation*. History of Science. New York: John Wiley and Sons. 1971. Pp. vii, 187. Cloth \$7.50, paper \$3.95.

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Santa Barbara, Calif.: ABC-Clio. 1971. Pp. 122. Cloth \$8.50, paper \$4.75.

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DOLLAR, CHARLES M., and JENSEN, RICHARD J. *Historian's Guide to Statistics: Quantitative Analysis and Historical Research*. New York: Holt, Rinehart and Winston. 1971. Pp. ix, 332.

DRUKS, HERBERT, and LACETTI, SILVIO R. *Cities in Civilization: The City in Western Civilization*. Vol. 1. New York: Robert Speller and Sons. 1971. Pp. 383. \$12.50.

FAIRBANK, JOHN KING. *The United States and China*. The American Foreign Policy Library. 3d ed.; Cambridge, Mass.: Harvard University Press. 1971. Pp. xvi, 500. Cloth \$9.95, paper \$2.45. See rev. of 1st ed. (1948). *AHR*, 54 (1948-49): 364.

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GAMSON, WILLIAM A., and MODIGLIANI, ANDRE. *Untangling the Cold War: A Strategy for Testing Rival Theories*. Boston: Little, Brown. 1971. Pp. xv, 222. \$6.95.

GLAUERT, EARL T., and LANGLEY, LESTER D. (eds.). *The United States and Latin America*. Addison-Wesley Ser. in History. Reading, Mass.: Addison-Wesley Publishing Co. 1971. Pp. iv, 204. \$2.95.

GODECHOT, JACQUES. *Les révolutions de 1848*. Le mémorial des siècles. Les événements, dix-neuvième siècle. Paris: Éditions Albin Michel. 1971. Pp. 502. Cloth 42 fr., paper 33 fr.

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- GRABOWSKI, TADEUSZ, and NOWAK, ZDZISLAW (eds.). *Integracja Ekonomiczna Europy Zachodniej i jej Aspekty Polityczno-Militarne* [Economic Integration of Western Europe and Its Political and Military Aspects]. Poznań: Instytut Zachodni. 1969. Pp. 273. Zł. 50.
- GRAY, RICHARD B. (ed.). *Latin America and the United States in the 1970's*. Itasca, Ill.: F. E. Peacock. 1971. Pp. xi, 370. Cloth \$10.00, paper \$5.95.
- GREEN, GIL. *The New Radicalism: Anarchist or Marxist?* New York: International Publishers. 1971. Pp. 189. Cloth \$6.95, paper \$2.45.
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- PENNINGTON, D. H. *Seventeenth-Century Europe*. A General History of Europe. New York: Holt, Rinehart and Winston. 1970. Pp. x, 486.
- PEAFF, WILLIAM. *Condemned to Freedom*. New York: Random House. 1971. Pp. 210. \$6.95.
- POMEROY, WILLIAM J. *Apartheid Axis: The United States and South Africa*. Little New World Paperbacks. New York: International Publishers. 1971. Pp. 95. \$1.25.
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- RYAN, GEORGE E. *Botolph of Boston*. Foreword by RICHARD CARDINAL CUSHING. [North Quincy, Mass.: Christopher Publishing House.] 1971. Pp. 268. \$10.00.
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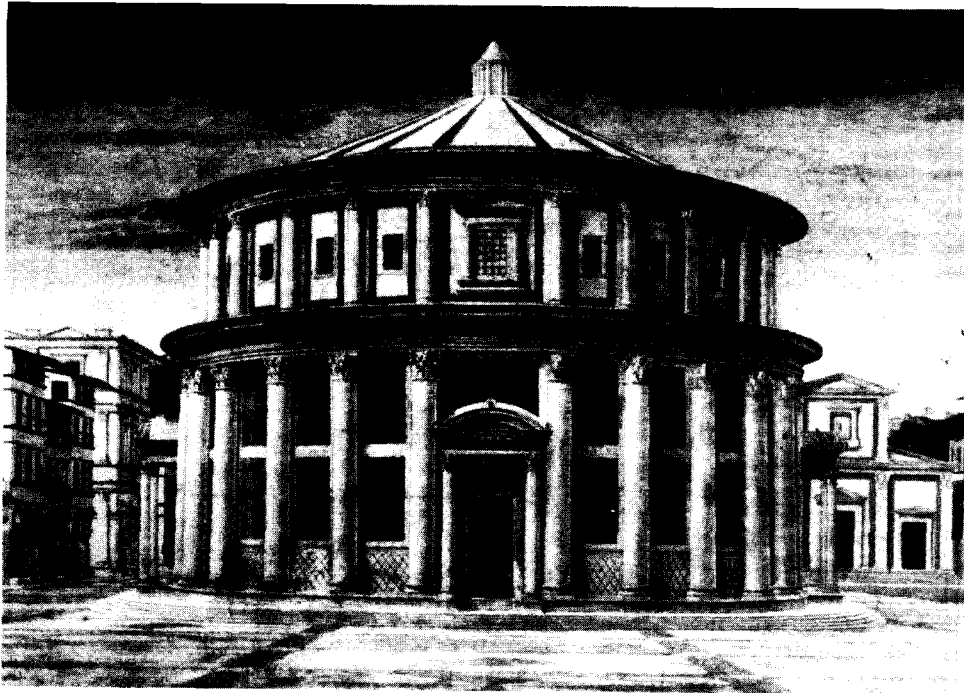
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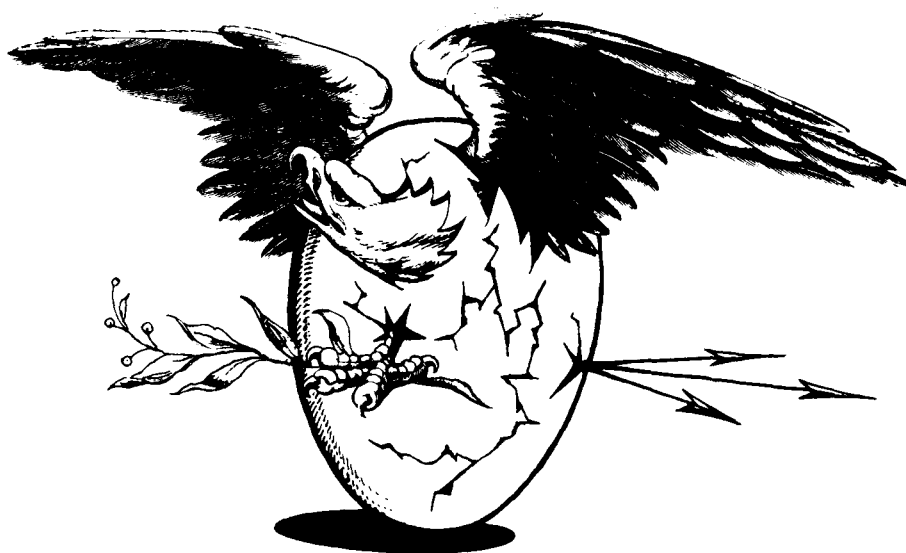
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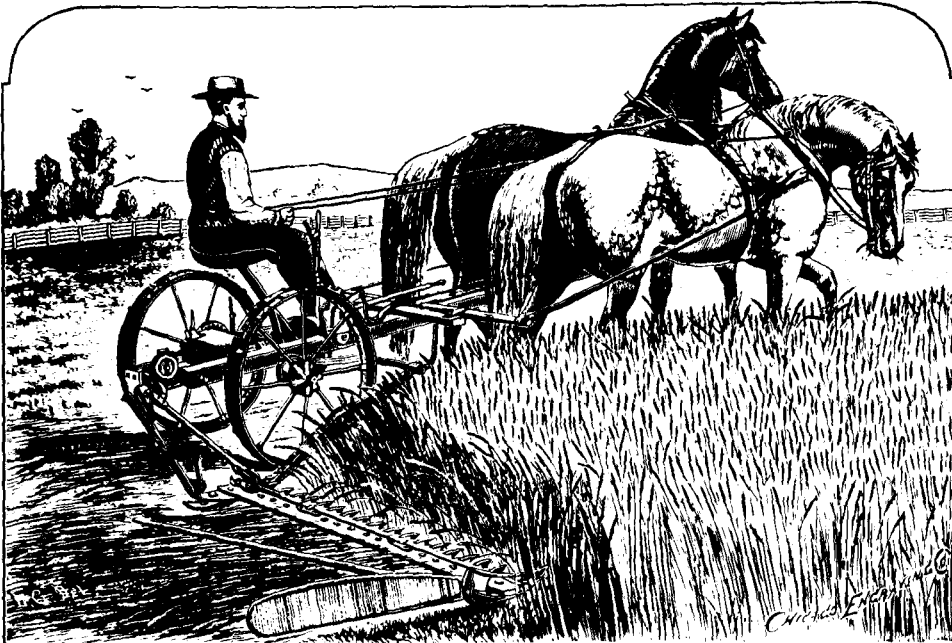
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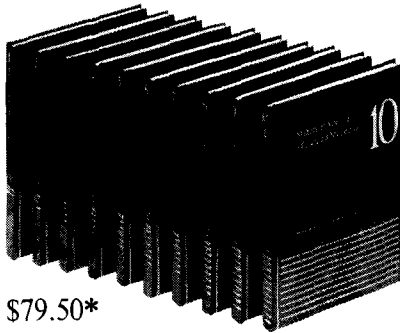
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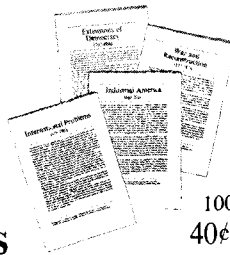
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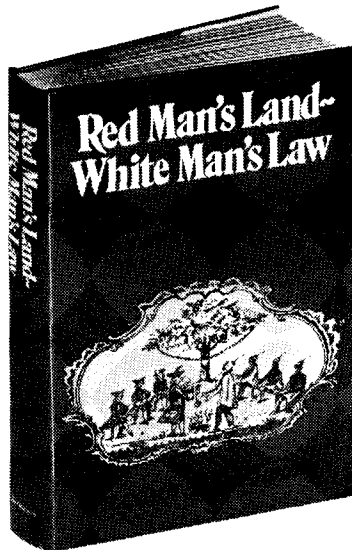
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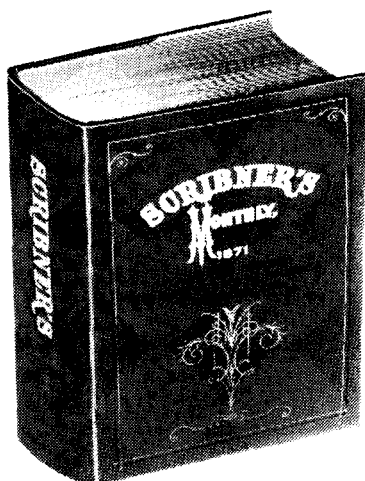
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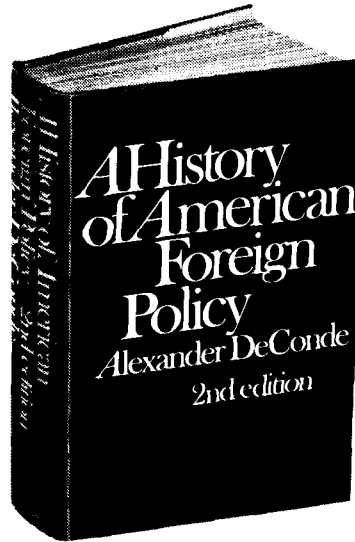
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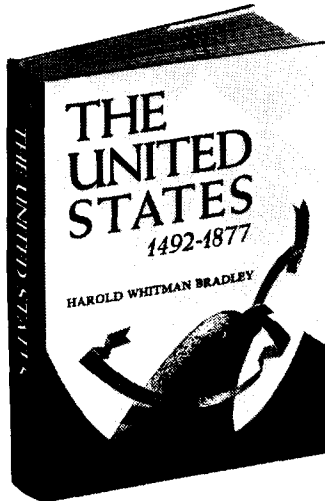
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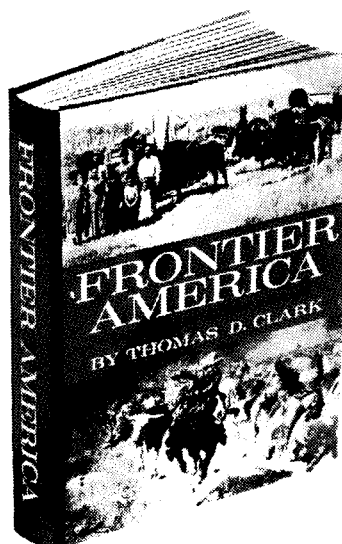
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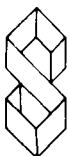
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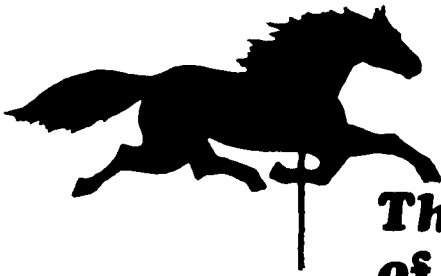
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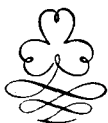
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